

Mead
Augusta Hancock Illinois Nov 7. 1848

My Dear Sir, your & Mr Carey's letter is recd &
for the remarks & corrections I thank you.

As to *Carex Willdenovii* I recd specimens & tickets from
Dr Short & Engelmann which led me into the error
But as soon as I saw the description of *C. Steudellii*
I corrected my labels & informed Dr Engelmann & Dr Hale
of the error sometime ago. The *Carex* which you
call *aristata* & another Mr Carey notices I will soon
overhaul my specimens & carefully examine & report
the result. Dewey called it *C. trichocarpa*. but it does
not look like a specimen I have from N.Y. I have
collected it in three places here & they may some of them
differ from each other. One form I supposed was *C. lacustris*
at first sight without examination. It grows in wet bottoms
& ponds. I have I think *C. vulpina* collected with *C. stipitata*
& *C. sparganioides*. Mr Carey says Mr Holton's were mixed
Mr Holton had his specimens of one I think & I suspect
Mr Sullivant & Dr Engelmann had specimens of me
mixed with others. *C. stipitata* or *sparganioides* -
Helianthus brachylefolius? I think can hardly be *H. divaricatus*
as the leaves are not sessile. Is it not a var. of *H. hirsutus*?
I had a specimen of *Astragalus distortus*, but did not think
my *A. pachycarpus*? was that - I have had but little time
for examining my plants. I have now more leisure
I can tell more about them, I could tell but little about
Carex till lately - *Heuchera hispida* & *richardsonii* you
think maybe synonyms? Your Truly -
I enclose some seeds
A B Mead

August 24.
Nov 8th

Free
L B Mead Pitt

Doct. A. Gray—
Cambridge

Massachusetts

Q. 8. What I called hem on the harynyne of what you
call ant aralia are trilla, white on the ridge ^{side}
near of the harynyne. constricthorose. leaves & stems
smooth. hem may be a different, some I collected in dry
forest place. I will look further at some 3 or 4
miles more putting up a large package of plants for the harynyne
a party desiderating about 2000 specimens is wished to collect for him
The next season - nearly all the botanist in the West have ceased
collecting.



Massachusetts

Cambridge

Dr. J. Gray

Free
Mr. Martin
August 13

August 13

Augusta Hancock Illinois Dec 11. 1858
My Dear Sir,

After a long time, I now renew my
correspondence with you. I have for some time past
been reviewing & arranging my Herbarium.

Our mutual Friend Mr J. F. Holton called on me
twice during the past season. While he was here,
I told him I would like to send you more plants, but
he said, when he returned East, it would be inconvenient
for him to take a package for you at that time.

I have many duplicates of some plants, such as *Carex*
Richardsonii & *Fraxinus quadrangulata*. & this tree
I did not find till the next season after you wrote
me. I met with several trees about 8 miles from Augusta,
I do not often meet with a plant which I have not before
seen.

Mr J. F. Holton told ^{me} he had a Volume of DeCandolle's
Prodromus beyond the one I have. The last I received from you
is "Pars decima Tertia" sectio posterior. & prima, I have nothing
beyond the amarantaceae & Nyctaginaceae. I would like
to possess the continuation of this work.

I have the 2^d Edition of your Manual with the Mosses &c.
I find several plants described in it, of which I have no specimens.
Dr. C. W. Short is the only Botanist who sends me regular ex-
changes of plants. I received a parcel of European plants
from him about two weeks ago. Dr Hale sent ^{me} a package
some time ago, since he removed to the ~~State~~ East end side of the
Mississippi, I can get no exchanges from any other Botanist
South of me.

Have you published the 3^d Vol. of your *Pl. Genera*? or anything
beyond the *Compositae* in your & Dr. Torrey's work?

I would like to obtain if possible, some more European *Carex*es,
which I have not, by exchange & other rare plants.

I have not seen or met with *Carex Houghtonii*. Dr Engelmann promised
me some more plants on his return from Europe. but I have
not heard of his return yet. Has your Botanical work been published by the Smithsonian Institution?

Please write me as soon as convenient
Yours very truly.

S B Mead

Oviedo Fla Nov 17/88

Prof. Sereno Watson,
Cambridge Mass }

Dear Sir:

I write
to ask in regard to a small
species of *Sabal* common in "scrub"
land around GUSTO & elsewhere
in this part of Florida. It differs
from the description of *S. Adansonii*
in that the spadix is reclinate
and much shorter than the leaves
usually resting on the ground & 12 to
18 inches long.

Although I have lived in this
part of Fla 7 years & have always been
particularly interested in palms, I have
never seen the true *S. Adansonii* (type)
growing wild. Mr E. H. Port tells me
that it occurs in shady hammock
instead of open "scrub" & is rare.

Of this form with short spadix is

not familiar to you I would be
glad to send you a living specimen
with roots, as I am about to get
a number to put in my Palm-garden
here. Has this form or species a name?

Another thing. I notice a
part of the plants of *Serenoa serrulata*
~~here~~ are very conspicuously glaucous
on both surfaces. of the leaves and
am told that in the Lake Worth
country this peculiarity is so general
as to give a distinct tone to the
landscape. Is this considered a
"variety," or merely an individual
peculiarity of the specimens that possess it?

I am trying to get together all
the species of palms that will endure
the climate here and have so far
about 120 varieties of which probably
half will consent to more or less
prolonged existence outdorm. I find great
confusion evident by comparing the
seeds sent to me by different dealers
under the same name and doubtless

have many times received the same
thing under different names. A
checklist, if such a thing exists, would
be a great help, are there any
such lists, either general or of
particular regions, that I could
obtain, either here or abroad?

If I can serve you in any
way by collecting or observation I ~~would~~
be glad to do so, in the meantime
my apology for troubling you with
these inquiries is that you are
the authority, and there is no one
else whom I can ask.

Very truly yours

Theodore F. Mead

Oviedo

Orange Co

Florida.

Summary of questions asked above:

1. Is the form described a var. of *S. adansoni*?
2. Is the glaucous form of *Serenoa serrulata* a
recognized variety?
3. Are there any available checklists of palms?

Bloxom, Accomac co., Virginia, March 3, 1891

Sereno Watson, Sir:

Dicksonia

Gray's Manual of Botany, Sixth Edition, I believe, restricts *Dicksonia pilosiuscula* to "New England to Minnesota."

While botanizing among the ferns here last summer, I found this species quite common here in moist upland woods. I inclosed some specimens to A. Commons, Wilmington, Del. who informed me that it was also common in his vicinity, but doubted whether it occurred in lower Delaware.

I suggested to him that he inform you of this matter; but as he did not let me know whether he would do so or not, I have taken this opportunity to write direct to you, as I wish to help make the Manual as complete, or correct as possible. The term, "Fragrant sweet scented in drying" is very appropriate as applied in 6th edition of Manual, to this species; while the term, "pleasantly odorous," in the fifth edition, was misleading, as the species is nearly or quite scentless in a fresh or green state.

Lepturus

Lepturus incurvatus is growing abundantly on "Borders of brackish marshes" and on brackish knolls on the bay side of this county. Perhaps I may inclose some specimens in the month of May.

I have become of late interested in the Hepatic
case, or liverwort, some of which are in flower or
about here at present. And here I would at
tribute to the omission of time of fruiting of the
most of the liverwort in the Manual, and ask
the cause of the omission. I am glad, however
that they are included in the last edition of
the Manual, and would, I think, be glad
that the classes been included also. Although the
liverworts have been but two or three weeks under
my notice, having never studied them before, I
find several species growing here, as *Doerjema*
calceolata? on rocks and rocks, and note in
fruit or flower. I find also *Althausia* in flower
here, *Althausia* — and several others not
in flower or fruit.

Yours Respectfully

Ellis C. Leach.

Geneva - Torrey Nursery
July 20th 1858

Dr. Wm. Gray
Dear Sir,

A few days absence has caused me to delay replying to your letter through my good friend Miss E. M. Morris.

It is very hard to decide in our nurseries between true species or mere garden varieties. I have some thirty species of *Spiraea* in my garden and it is a very difficult matter to fix characters for them. I have to day a kind opened its flowers, marked "*S. Billiardii*", which I had supposed to be "a variety of *Salicifolia*", but now it is in bloom is so intermediate between the European and American forms of *S. Salicifolia* that it is hard to say where one begins or the other ends. Horticulturally speaking, the fine pink flowers of *S. Billiardii* is its chief distinction.

In a cultivated state the marks that characterize varieties become fixed characters to a great extent. Many things we know to be but varieties reproduce themselves exactly again from seed. I suppose the rule will be that where kinds very nearly resemble in their chief characteristics they should be set down as varieties merely?

But then what would become of *Platanus occidentalis*, and *P. orientalis* which in practice we can only readily distinguish by the bark of the latter being lighter in color in the young shoots, than it is in the former? And yet it is so essentially different as to be able to resist a malignant disease which follows the other - the elm - all over the world.

It will be very convenient for me to collect specimens of oak for you this year - I have to collect some hundred birchbolls for Kinosmen on the continent of Europe. I shall save them for you with great pleasure. I received last year a few acorns of what I took to be *Quercus blanda* from Ohio. They appear now growing to be exactly like *Q. macrocarpa*. I think one year old seedling oaks can be more readily distinguished than they can be ever afterwards by their mere foliage alone. I have paid great attention to oaks for several years, and yet feel sorely puzzled at times. For instance in collecting *Quercus coccinea*, I have invariably placed great dependence

on the color of the early ledons to distinguish it from *Q. tinctoria*. Yet in Southern New Jersey, where the acorns still have all the characteristics of *Q. coccinea*, the foliage and general habit so approaches *tinctoria* that I hesitate. The Jersey scarlet oaks, I have never seen turn scarlet in the fall, as they do in Pennsylvania. In the specimens I get for you, I will try to save at the same time leaves & shoots, and preserve the name of the locality they come from.

I am commencing to get up a stock of Australian plants, and should be very much pleased by a set of the Australian seeds you have received. Independently of this I should be glad to make up a collection of the shrub seeds for Dr. Muller. I can promise you from a hundred to one hundred fifty species for this purpose.

It is quite convenient for me to collect a few seeds or specimens of most of our trees and shrubs - I have a great desire to have - not only of these - but of plants of the whole world - Tropical

or otherwise, as complete collections
under cultivation as I can get. So
that I try to have parties to collect for me
wherever possible. I have very much
yet to add to my small beginnings
and should be glad to receive at
any time a few of any seeds that may
come into your hands for distribution.

With great Respect

Yours very truly

Thomas Meehan

Miss Morris informs me you are examining
specimens of Japanese plants. I have
a *Catalpa* from Japan Market Co.
Kaempferi. The leaves are much more
attenuated than our species, but in
other respects much resembles it. In
case you should have specimens of it, it
occurred to me to enquire, whether the
flowers were very different from ours?

The Germantown Nurseries,

Chew Street, below Gorgas,

Germantown, Philadelphia.

June 7-1878

Dear Dr. Gray

I trust you will forgive me for saying that I have never had any ambition to be styled a "Professor". I have never had the slightest hankering after honor or credit, - nor have I ever written a line in my life in praise of myself, nor to fish for praise. I have never had any ambition to be considered even a Scientific man, as the phrase goes. My parents could not educate me. They had twelve children to bring up on twenty shillings a week. They had eight when I was twelve years old - having had three children at one birth, and then had but 12 shillings a week. A neighbor paid 10

Shillings a year to entitle him
to a vote to send me to a
charity school. I went to
work for 2 shillings a week
at 14, twelve hours a day
and have been to hard work
every day since that I was not
sick. It would be a ridiculous
thing for me to ape the "Professor"
or in any way to ask for honors
which educated men might
justly claim. The only ambi-
tion I have had in life is to
make hard work honorable,
and to show that even man-
ual labor may be intelligent.
When I wrote my little hand-
book of ornamental trees -
about 24 - it was four times
its present size. I could
find no publisher in New York
or ~~Down~~ Philadelphia. The
quies was always "Who are
you?" Lippincott at last agreed
to let his name go in the
book, if I would bear the expense!
I had only \$300. in the world. I
could not stand the expense of

The Germantown Nurseries,

Chew Street, below Gorgas,

Germantown, Philadelphia.

So large a work. I cut it down three bounds to its present size, - but as it was my own venture, I answered the question who is Morris Meekins by simply adding "Gardener".

When D. R. King started Re Gardener's Monthly, he was strenuous for a high sounding Greek or Latin title, - I would wish it in no other condition than simply a "Gardener's" paper. The long handle of "graduates" &c on the title page, I was finally overruled to permit. It was not my wish. It was contrary to my desire.

In the "native flower" prospectus there was a long list of titles when submitted first to me. I struck the most of them out. When it was finally submitted

As to me, I struck out the
most of those now there. But
no attention was paid to them.
I feel that perhaps the publishers,
like my early experiences, want
to meet the public expectation
But only little men can
write intelligent books, and
I regretfully give way to
them as a matter within
their business. Our members
say why did you not serve
"Vice President of the Academy"
Simply because I did not
say even the other. The
Publishers had not found
it out, and I was not
anxious for it.

You see I shall per-
sonally not be at all
grieved that you cannot
afford to speak as well
of my work as Professor
Sargent does. I think

The Germantown Nurseries,

Chew Street, below Gorgas,

Germantown, Philadelphia.

You have never said one word in public of praise of any thing I have ever done. Had I been sensitive about honor or credit or praise for my works, I might have given some thought to this. But I have been too conscious of my own deficiencies, too anxious to learn, too thankful for the corrections that have often enabled me to improve little by little, to let that fact remain long on my mind. I must be candid then to say that you should praise my work on Nature flowers, never once entered my mind. For my publishers take who have been induced by the pressure of partial friends to push the work on

Meehan

me, ~~I~~ and not by my
own seeking, I shall be sorry
if you bear too hard on them.
They have risked ~~as~~ great
deal - I have no money
to lose, nor any thing to
save. I have all the money
I want, and all the
honor I want, but it is
~~different~~ different with
them. So please be
as tight as you can with
them, but so with me just
as you like, - only spare
them. I wish somebody
better fitted for the work than
I had been found for their
sakes. In this I have
only done, what I have al-
ways endeavored to do - to
be as useful as I knew
how in the situation felt
I owed to the trust me.

Very truly yours

Thomas Melham

any way in seeds
this year. It is so
little trouble,
while collecting
seeds for my busi-
ness to get a
few more than
I want, and I hope
if you have any want
of any thing that way no
delicacy will prevent
you from offering me
the pleasure of presenting
them to you.

Very truly

Wm. Meehan

German town

June 21/95

Dr Asa Gray

My Dear Sir,

By this mail

I send you a box containing
a flower of a beautiful
Cactaceous plant from
Mexico, which if not al-
ready described, I think
very prone a new genus.
It has a very anomalous
manner of growth. The
root is tuberous. Six or
eight tubers about 1 1/2 inch
long each, and shaped

like Dabbia tuberos, pro-
- ceed from the Collar of
the plant. They increase in
size annually. The
stem is woody, looking
like a stunted pine,
about as thick as a
pipe stem. The succulent
branches come out at
the top. ~~like~~ There is
in German collections
an "Opuntia tuberosus"
which I have not seen,
but the spiral stamens,
and short contracted
tube is different
from Opuntia.

Though the succulent

Stems are round and
have the spines arranged
as in Cereus, - the flowers
are permanent, and
open and shut in
the sun as in Opuntia.

I have been very
fond of studying
Cactaceae, and have
now living specimens
of over 200 species.

I am pleased to
say that I received
a great many of Dr.
Mueller's seeds. Many
are now blooming.

I shall be pleased
if I can serve you

Printers and Lithographers
were almost on my heels,
before the chapters were
complete. I have to tell
you this that you may
understand how there
was some "Paste" in
this part of the work which
you know always favors
blundering.

Now, secondly. — I remember
well, that as I was writing
that chapter, pen in hand,
it occurred to me that
~~an examination~~ an exact
definition of legume from
some popular glossary,
would be very much to
the point, and your work
lying on the table waiting
a chance to read carefully
for review, I took it up.

The Germantown Nurseries,

Chew Street, below Gorgas,

Germantown, Philadelphia.

Feb'y. 18 - 1880

Dear Dr. Gray,

I did not go into details
as to "how I fell into the blunder
of copying" your error, because
the way in which people get into
these troubles, are generally
"too wonderful" to tell. But as
you seem interested in knowing
the particulars, I will tell you
as near as I remember, though
it is rather a long story.

First I must say that
when our first series was
being issued, I felt kindly to-
wards others who were
working in the same field,
and would not have
many fears because I did

not want to interfere with
Eaton's work. At the conclusion
of my first series, I had all
ready for the second (M.S.S.)
when the publishers told me
from the "numerous complaints"
from subscribers, the lack of
ferns was seriously endangering
the success of the work, and
"I must have more." Now it
so happened that though I seldom
allow any personal considerations
to influence me, I could not
help remembering that while
I was always liberal with my
works to other friends, they
rarely showed any considera-
tion for me. I have noticed,
kindly I think, in the Gardener's
Month and elsewhere Eaton's,
Sordani's, Robinson's, Williamson's,
Gray's, and others' works, but
all purchased with my own
money, except in one

single instance - Gray's
Structural Botany - while
in addition to sending my
own works freely to ~~them~~
many of them; and, generally,
except in your case not
even an acknowledgment
of the receipt of the work, -
certainly never ~~with~~
with an acknowledg-
ment of the literary
notice made solely at
my own cost. I think
therefore I may be pardoned
if I let personal consid-
erations decide that I
might for once, go on with
more ferns notwithstanding
Eaton's work. But I
had no ferns drawn, and
I had to wait till the

The Editor wrong and the
original author right.
I find there is no better
way to lose some valued
friendships than to seem
in a hurry to point out
errors, for what is
intended as kindness
is taken for censorious-
ness. Even in Prof. Watson's
excellent and remarkable
conceal bibliography I find
occasional mistakes. I gave
him one or two at first; but
finally his continued my
opinion. I have been
also sorry, since I saw how
my intentions were received,
that I corrected Jones' notion
about the "poisonous" malva-
trum coccineum.

Very sincerely,
Mona Freeman

The Germantown Nurseries,

Chew Street, below Gorgas,

Germantown, Philadelphia.

and turned to the Glossary,
and saw what I have
quoted, and I well remember
that I was struck by it
and that a ~~fast~~ pea-pod
came at once in my mind
and that I remembered
that it did burst first
on the ventral face, and
I thought I remembered
that that all legumes
did burst first in that
way, and that when the
dorsal suture separated
it was only by drying after
death, and for the moment
I thought this was what
you intended by the
word "normally"; and,

without reflecting any
further, the whole subject
passed from my mind.
It is not often that I can
remember to tell you "how"
these things occur, but I
happen to be able to do so
here. Of course I am
not justified by this
reasoning, - "if" as you say
I had looked to the reference
~~to~~ the body of the work, "if" I
had thought more of "what-
mally", - or "if" something
else, I should not have
"gone and done it".

I will not ask you
how you made the
blunder in the first
instance, - but I have

no doubt there are
some "ifs", which made
the blunder natural
enough, - or you would
not, with your usual
great care, have done it.

You say you are
"obliged for calling attention
to errors". I believe you
are, - I am sure I am, -
but I have not done
much of it with friends
lately, except where I
find it comes in the way
of some of my own work.
Where contradictions occur
it becomes necessary for
me to show them, or
the people will think

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Germantown, direct to Upper
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tion, is ten minutes' walk from
the Nursery.

Germantown Nurseries,

THOMAS MEEHAN, Proprietor.

Office, Upper Church & Chew Sts., Germantown,

NEAR PHILADELPHIA.

788

Dear Dr. Gray

If there is any error
about the *Erigeron mucronatus*
it is not as you uncharitably
assume the result of
"recklessuess".

I had to let Mr. Lurzer make
some drawings under the eyes
of friends - as I supposed - in
Cambridge, while I was in
Europe ~~three~~ years ago. This
was given him to do with
others. It was put in with
my first set, but I rejected
it, because "I did not
think it was ever found
in the United States". I

wrote to Mr. Lurzer to
inquire of the botanists
of Cambridge whether they
had not made some

Mistake. He wrote to me
some time afterwards that
he had followed my instruc-
tions, and that they reported
that the seeds had been
actually collected within
the United States boundaries.
This was two years ago, and the
Chapter written at that time
and the matter passed from
my mind.

If it had been more
recent and since I have
learned to mistrust
the accuracy of almost
everything, I might have
inquired more particularly
as to exactly who the botanists
were who were consulted,
but you see I was "green"
then and am suffering for
it now. - Wm. T. Meehan

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Germantown Nurseries,

THOMAS MEEHAN, Proprietor.

Office, Upper Church & Chew Sts., Germantown,

NEAR PHILADELPHIA.

Feb. 24 1886

Dear Dr. Gray,

I have to thank you for your
kind and good letter of Feb. 24.

No one is more anxious to have my
work as near perfection as possible,
and I always value all the
help you can give, and have to
often given me. I don't expect
to have it perfect. I expect to
make blunders — sometimes
ship it blunders, — and thank
I must say that I sometimes
have thought the charges you
sometimes make of "haste",
"recklessness", "carelessness"
and so on, undeserved, — I must
confess that your efforts to
help me have been of
immense service to me,
and I am quite sure
I increasingly appreciate

The necessity of extreme
caution, — and in a great
measure have I derived
this increased caution from
the letters you have
written to me.

Do not believe dear
Dr. Fay, that your ink
has been wasted on me.
I think you sometimes
needlessly sharp, — some-
times that you expect more
from me than you would
from others engaged in my
work, — but I am always
thankful for ever help,
even though roughly attended,
and most assuredly I
have to thank you for
numberless for diffie,
and beg for more.
Ever truly yours Wm. McLean

The Germantown Nurseries,

Chew Street, below Gorgas,

Germantown, Philadelphia.

Dec. 14 - 1887

Dear Dr. Gray

I cannot tell you
how much pleasure it
gave me to see your
kind writing once
more.

I have replied to
Miss Bowen, - but unfor-
tunately not to say in
her satisfaction, - I had
at the same time a card
from her. She has 1st & 2nd
series Z. & Z., and is looking
for the 3rd. The M. S. S.
and drawings are ready.
3rd series was to appear
in January 1882, but
the publisher died suddenly

Last September, and his affairs proved so confusing, that the administrator has been unable to settle them yet.

I have been tired of waiting for some one to buy the copyright from him and for the last two weeks have been looking for a new publisher, but without success. I have found one who is willing to "try" another series, but only at the rate of 2 plates a month, till another series is complete, but I do not care to accept any such slow

proposition. The past two series have been successes, and it seems the time for "Experiments" is passed, and I should like it continued now, as a ~~regular~~ regular monthly serial somewhat as is the Botanical Magazine.

But I find the great trouble is to find a publisher without the three years experience I have had, to see it as I can who have had it.

Very sincerely yours
Thomas Meehan

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Germantown Nurseries,

THOMAS MEEHAN, Proprietor.

Office, Upper Church & Chew Sts., Germantown,

NEAR PHILADELPHIA.

March 12 1881

Dear Mr. Watson

I send you by mail
stem and flowers of a lily
apparently of the longiflorum
type, which I have begged from
a neighbor who insists that
several years ago a lady
friend of his found it wild
in Florida, - though another
says the lady told him it
was wild in the West Indies.
It came here from a traveler
from Florida at any rate.
I have made no attempt
at looking it up, as I thought
your investigations into the
order would make the
matter more certain.

It grows only about 2 feet
high. Sincerely yours
Thos Meehan

Meacham

Cars leave Ninth and Green Streets,
Philadelphia, every hour.

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Germantown, direct to Upper
Church Street.

Horse Cars along Eighth Street, direct
to Upper Church Street.

Chestnut Hill Cars to Gorgas Lane Sta-
tion, is ten minutes' walk from
the Nursery.

Germantown Nurseries,

THOMAS MEEHAN, Proprietor.

Office, Upper Church & Chew Sts., Germantown,

NEAR PHILADELPHIA.

July 1 1881
Dear Mr. Watson

A Correspondent from
Woburn, Ontario, sent me
three years ago a tiny rock
of a plant he found near
there. He had never found
but this one himself, but
had been informed that it
was often found in Man-
toulia Island. It has only
flowered now, and proves
to be a variety (scarcely that)
of *L. Martagon*. I thought you
might like to know it is found
on this continent.

I flowered the rock of
Lilium grayi I brought from
Rosa Mountain last year.
You do not place it in the
pendulous section (with the
Canadense), but I find no

difference in this respect.
except that the leaves (perianth
leaves and all) are shorter
and broader, it is difficult
to find any difference that
can be described. Of course
the color is brighter than most
of the "Canadian" lilies. It
was in flower June 10th. The
"Canadian" from New Jersey
growing beside it ~~from~~ opened
June 20th, and I have some
from Miss., which will have
their first flower tomorrow.
I have been watching and
taking notes of the growth
motions of lilies this season
and find it very pleasant
study.

Sincerely yours,
W. P. Melvin

Chestnut Hill Cars to Gorgas Lane Station,
is ten minutes' walk from the Nursery.

Meehan's Nurseries.

Nov. 6 1885

Dear Mr. Watson

I thank you very much for your suggestions about the *Arum*. An Editor has continually before him the image of the "devil" knocking for more "copy", and is apt to write hastily and therefore erroneously. I always try not to forget this liability to error, but sometimes get caught for all. And in this case I was sure that Michael had distinguished a var. *candida* = *Arum* *Atarvaco* var. *candida*, and was amazed after reading your letter, to find nothing of the kind, either in Mex. or anywhere else! The *A. candida* Schult. I had not in my mind, and thought the one in English culture (it is not in America) was this light colored *A. Atarvaco*. I will correct the error in an

early number.

Aside from this I think there ought to be some distinction between the two forms that a florist can reach. The best way, note flowers white with a faint cerise tint, does not begin to reach the beautiful dark shade which I suppose was in mind when "flowers purple" of Barron's was written. There is surely a wide gap between this and Morrison's "white", and Pursh's "rose color". I have never had the two forms growing side by side, so as to get the exact comparative differences, but my recollection is that the red or purple atamisco has quite as much difference from the white, as the Treats has from the white. Indeed all three seem to me to run so closely together that it is not easy to separate them.

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OFFICE, UPPER CHURCH AND CHEW STREETS, GERMANTOWN,
NEAR PHILADELPHIA.

Meehan's Nurseries.

THOMAS MEEHAN, PROPRIETOR.

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In regard to the *Polargonium*
question, there is no attempt
by florists to separate them
botanically, - they simply re-
tain the names in use a
quarter of a century ago,
because no good common
name has been introduced
to supplant what they once
knew as "*Geranium*" -

"Zonale *Polargonium*" is the
term that obtained the greatest
popularity, but this was not
near wide enough, and
now all the forms used
for bedding purposes, since
which have been derived in
any way from the old
so-called "*Geranium*", are

called *granatum*;
Melargorum being assigned
to the Spring flowering, and
very unequal-petaled class.

Glad to believe you
have got wholly over
your Southern fever
Sincerely yours

Thomas Meehan

Meehan



Dear Mr. Watson

July 4 1887

I am glad that I sent you the hasty note. I had failed time and again to see when and how the pistil elongated. Satisfied that it occurred at night, I cut specimens and placed in a tumbler of water, and the work was done therefore by "midnight lamp". I spent the whole evening in studying the development. Since getting your letter, I have gone over the prisms still in the tumbler, and think you must be right about there not being a pair of connate anthers about the stigma. Yet the attendant phenomena ~~are~~ ^{are} very curious. As the pistil emerges from the opening prism a direct view of the apex

Gives an outline exactly of
two ~~columns~~ authors back to
back 38, and the side view showed
what appeared to be the exact
organic outline of the stamens.
a pair of very small compasses,
placed under this mass lifted
it off exactly as a cap, leaving
the clear oval stigma exposed

 of course it was only assumed
that these "being authors" the
flaments must be entirely
consolidated with the style.

Now, since getting your letter,
I feel that the pistil in its
growth has carried with it
the whole mass of pollen
from the anthers, - wiping
them out so completely
as to make the the
true anthers seem barren, -
and so nicely as to preserve
the outline of anthers on the



~~on the~~ elongated
style.

The elongation by the
way, is complete in 30
minutes. It commenced
with me after dark.
I have never seen
any thing in the day
time, and it was
this failure that led
me to take it for
night observation.

If not the ⁴ Quilling
hook, it is at least
a remarkable arrange-
ment for self-fertilization,
and the whole
series of observations is
very curious.

Sincerely yours

Thomas Meehan



OFFICE: UPPER CHURCH AND CHEW STREETS,
GERMANTOWN, NEAR PHILADELPHIA,

July 22 1887

Dear Mr. Watson

Iack you a correspondent
in Alabama, with a botanist in his
own estimation, but knowing
plants pretty well, sent me a
dried up stem and leaves of a
Lilbiscus, which he said was
growing only in limited locations,
and different from other Lilbiscus-
cuses that he had seen. I could
not make it out, and sent
the rough pieces to Dr. Gray,
who was also uncertain, and
advised me to try to get a
root and grow. I managed
to get a poor root; but it
will make two flowers. I want
to send you one. Would you
prefer it dried? or fresh.
The first of the two will be
open to-morrow. It appears
to be a deep straw yellow.

The stem is smooth and
glaucous beneath after
the manner of *H. militaris*,
but the leaves, you see,
are very different.

The flower is set
very far on the petiole.



and is in this respect different
from any I know. I need
cut and send you the whole
stem, but want to see
a seed vessel.

Yours

Thomas Meehan



OFFICE: UPPER CHURCH AND CHEW STREETS,
GERMANTOWN, NEAR PHILADELPHIA,

Aug 1 1887

Dear Mr. Watson

As Dr. Gray is away, I must
batter you with my observations.

Did you know that two mono-
delphous stamens enclosed the
whole pistil in *Cephalanthus*
and that the four stamens
of the corolla tube are wholly
sterile?

You have of course seen
the little black gland on
the left hand edge of each
lobe of the corolla.

Did it ever occur to you
how analogous it is with
the glandular chaff (four
to each "pore") "file" at the
base of each flower?

With all a "misty
link" between *Verbena* and
Compositae?

I regret that the flowers

sent (in a separate
newspaper) are the last
of the season -

You want me
with "porets" just opening
to learn the math.

Sincerely yours

Thomas Meehan

THE GARDENERS' MONTHLY AND HORTICULTURIST,

THOMAS MEEHAN, EDITOR.

\$2.00 a Year.

CHAS. H. MAROT, Publisher,
814 Chestnut Street.

German town
Philadelphia,

Jan'y 15 1888

Dear Mr. Watson,

Your correspondence must be so enlarged through Dr. Gray's illness, that I hate to make one more in the burden, but *Echinocactus papyraceanthus* will not wait. I want to ask you move about it.

I have an European correspondent who is willing to pay well for new Cacti - so I set Siler to hunt *E. papyraceanthus*. He finds one at length, but with a bud that did not seem at all likely to be "white" as described by Engelmann. I thought best to defer sending to Europe till it flowered. It is now in bloom, and I send you the rough diagnoses I have made of it.

If you recognize it, all right. Now I must send you the plant, - and if you want it for science, of course you shall keep it, for science before cash any day.

I also enclose specimen of a plant raised from seed sent me from China. Is it *Spergulus asiaticus*?

I hardly think quite this, but I do not know who else.

The publisher of the Landowners Monthly died a few weeks ago. The family (widow and young ladies) would have continued it if I said so, - but I could see no one to manage it as the father did, and I feared what is a valuable property now, might be wrecked in the future. An offer of \$5000 cash coming along, I advised them to take it. It throws me out of "bread and butter" and a task I had come to love, - but as my Flowers and Terns, is now going on, and will probably not stop during my life time, again, I might perhaps to rest after 30 years steady work on one thing.

We hear every few days, through Redfield of Dr Gray. Sorry indeed that all is so utterly hopeless.

Very truly Yours
Maurice McMan

OFFICE OF
THE GARDENERS' MONTHLY
AND HORTICULTURIST,

THOMAS MEEHAN, EDITOR.

\$2.00 a Year.

CHAS. H. MAROT, Publisher,
814 Chestnut Street.

Philadelphia, _____ 1888
Lancaster, April 28

Dear Mr. Watson,

Long ago Dr. Gray
wrote to me that he was under
an agreement with Professor
Pursh to write to no other
agricultural or horticultural
journal but the American
Agriculturist, — that therefore
any note he sent to me
must appear anonymously
or he should not send them.
This was because I did use
his name to a short piece
supposing he would not object.

I could find you references
to a good number of
paragraphs, — but I doubt

Whether it would be
fair under the above
understanding, to use
them as his.

However should you
think differently I will
with pleasure go over
and put them up.

I have a large amount
of private correspondence
relating to Physiological
and Morphological matters
chiefly — both from him
and Dr. Engelmann,
but these are hardly ever
of value till a hundred years
or so, when they sometimes
throw light on contemporary
events in which the world
did not know there was any

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814 Chestnut Street.

Philadelphia,

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interest till long after.

Many thanks for the last paper of Dr. Gray which you kindly sent to me. It had a special interest to me from my having a few years ago insisted on some distinctions between the Colorado form of *Asperopsis purpurea* and ours. At first he agreed for its identity with Buckley's *A. heptaphylla*. Finally satisfied that my position was correct he wrote (I am trusting from memory) "I think it doubtful if I shall be spared to reach the revision of *Vilceae*, for

the Gynopteral Flora, - but
if I do it shall appear
as L. G. var. laciniata!
Naturally I was struck by
his coming so near to
reaching it.

Very truly yours

Frederic Mearns

I think the earliest public notice
of Dr Gray's services to Botany, is in
the preface to Beck's Flora, -
but no sketch of his life seems
to have noted that.

I feel keenly the loss of the
Monthly. It would have been
a sad, but yet gratifying task
to me to have told of his great services
to horticulture.

ESTABLISHED 1852.

Meehan's Nurseries,

OFFICE:

UPPER CHURCH AND CHEW ST.

Germantown, Phila. Oct. 2 1889

My dear Dr. Watson,

I spent the whole afternoon through the park of my collection my wife has been poisoning, &c, without finding a solitary Amaranthaceous plant of any kind!

When I started in life, I determined, some day, to have a first class herbarium. Through life I have been simply collecting specimens to make one with, — all in alphabetical bundles for convenience in putting away. The natural order, genera and species were all in alphabetical sequence.

Amaranthaceae should, therefore, be in the set, she has been at work on the two past years, — and where it has gone to I cannot imagine.

The fact however remaining
that I am unable now to
confirm the Chadstone location,
though I have no doubt
whatever of its existence as
reported, because I carelessly
compared it, at the time,
with specimens in Herb. Acad.

As I have said it might
have been an introduction (from
Arkansas Colorado?), as it was
on the line of the Kansas
Pacific, and ~~at~~ ^{near} a station
of ~~the~~ one of the incipient
towns, yet deep wild desert,
but spring up along there.

It was on the collections
made on the Rocky Mountain
excursion after the St. Louis
meeting of the Am. Association
that I found it.

However, we can all
be mistaken, and if
there is likely to be any

ESTABLISHED 1852.

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UPPER CHURCH AND CHEW ST.

Germantown, Phila. 188

doubt about it, much all
reference to it, - or if worth
noting at all, put "doubtful"
after the reference.

Thanks for your
examination of the Cardinal,
I now feel satisfied that
it must be C. hirsuta
as your material must
be so much fuller than
ours for examination.

Very truly yours

Thomas Meehan

COMMON COUNCIL
PHILADELPHIA.

Communications, Apr. 20. 1891

Dear Dr. Watson.

The enclosed *Leptoglossus* is from Albany, N. W. Texas. Rather hurriedly to day, Mr. Redfield and I decided it was not in *Hesperia*, nat. *Sierrae*, and for want of fruit, could not decide where to place it. It was voted unanimously to "send it to Watson". Looking over your monograph to night in trip, I fancy it may be *A. Shurtlandii*.

It was also "voted" to send you the enclosed *Abronia* as I say, but which Mr. Redfield doubts because of the scrovy alternate leaves, and apparent want of involucre. The alternate leaves

certainly are strange, but
the tendency to triplinerve
and general character
otherwise, I should think
would place it there.

This a scrap my wife has
just brought from Santa Cruz
on the Pacific coast. Redfield
doubts whether the alternate
leaves ~~are~~ can possibly
belong to the flower if the
flower be an Abroma,
but Mrs M. seems positive
if they do. — There are no
such leaves on any of
our specimens.

Sincerely yours
Wm. M. Mechan



RED-FLOWERING DOGWOOD
CORNUS FLORIDA FLORE RUBRO.
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NURSERIES.

OFFICE
UPPER CHURCH AND CHEW ST.

Germantown, Phila. Apr 25 1891

Dear Mr. Watson,

Our young men have placed
the herbarium on the list of subscribers,
and are very thankful for it.
It is rather a heavy undertaking for
them, and they are as yet uncertain
how far they may go in free
distribution. I have always
felt that the good which Cambridge
does for botany entitles them
to every thing free. I hope we
shall reach the pecuniary
stage when we may do so. Our
prospects are good; - indeed I am sur-
prised that so many should subscribe
before seeing the work.

I am sorry I mislaid the Alumina.
I do not know what became of it.
Very sincerely,
Thomas Meehan,

Neck

THOMAS MEEHAN.

THOMAS B. MEEHAN



RED-FLOWERING OOGWOOD.
CORNUS FLORIDA FLORE RUBRO.
COPYRIGHTED 1880



NURSERIES.

OFFICE
UPPER CHURCH AND CHEW ST.

Germantown, Phila

Oct. 13th, 1891

Dr. Sereno Watson,
Cambridge,
Mass.,

Dear Dr. Watson:-

Our Academy Expedition, which accompanied Dr. Peary to
Whale Sound, has brought back a small collection of plants from McCor-
mick Bay and Disco^{graph}. We have enough to make several sets, and in the
interest of geological botany, we presume you would like to have one
set, which it would be a great pleasure to us to send you. We have
some four items, which we cannot positively identify as to the proper
species. I enclose you them in this envelope. Would you kindly decide
for us what they ought to be. You may return them, as we shall have
better specimens for you in the set which we propose to set aside.

Very truly yours,

Thomas Meehan

McEhan

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Office, Upper Church & Chew Sts., Germantown,

NEAR PHILADELPHIA.

Dear Mr Watson

188

I said last year that Martindale
had told me he had *Titularia*
Kamtschensis from Canada
by Macoun; and this you
doubted.

I have only just had
the chance to see ~~just~~ how it
is, and you are right in
the main. The label reads
4 *Flora Canadensis*.

L. — K —

May 18. 1875
Valley of Boyer River, B.C.
Macoun

No. 1232. "

This scarcely brings it into
"Canada" as we popularly
understand it. Perhaps
Kunth had the same broad
idea of "Canada."

Yours Truly, M. Meehan

Basel, 21. Aug. 1839.

Dear Sir,

at my return from a journey to the southern parts of Switzerland, a few days ago, I found your lines which you had left at my house with our friend Annot's note. It is impossible to tell you, how much I did and ever shall regret that your visit to Bale should happen during my absence, thus depriving me of the pleasure of making your personal acquaintance ~~of~~ which I was long ago most desirous, and to which I have looked forward with so much anxiety since last spring when a letter from Annot announced me your approaching visit. Is there no prospect of your returning to Switzerland before leaving Europe? Having not heard of your having been at Geneva, nor at Bern where you would no doubt have visited Mr Shudler, a most zealous Protestant, I conjecture that you have not been in the interior of our country, and therefore I still entertain some hopes that you will one day return. If so, allow me to beg, as a great favour, that you will again take your way through Bale, and inform me by a few lines of the day or week when you think to arrive here.

Having long ago wished to get connected with some Botanist in the United States, nothing could be more welcome to me than your kind proposal of forming a botanical correspondence with you, and you may depend upon my exerting all in my power to satisfy your desires. Had I been so fortunate as to see you here, I would certainly not have let you depart without a good parcel of such plants as my duplicates afford, chiefly from Switzerland, Savoy and France, - but they are still at your service, and in case you cannot come to fetch them yourself, you would do me really a favour by indicating whether and where I may have the pleasure of sending them to you. I hope you will have the life scriptures for accepting my offer, as I myself would be very happy to receive plants from your country in exchange, and, indeed, as soon as I shall have a favourable answer from you, I shall send you, with my plants, a long list of my desiderata of North American plants. Most of the more common species of the United States I possess already, particularly from New York, Pennsylvania and Ohio, - but in the plants peculiar to the southern and more western countries, as also from Canada, I am very poor. At all events,

whatever plants you would send me, whether new to me or not, would be most valuable to me as authentic specimens ^{from} ~~of~~ one of the celebrated authors of the newest, and undoubtedly the best, Flora of North America.

And now, after having exposed with so much freedom, if not indiscretion, my wishes, let me entreat you to acquaint me as frankly with yours! With the hope that these lines may find you still at Berlin and in good health, and that they may be the beginning of a long correspondence, equally agreeable and useful to us both.

I am, dear Sir, with
perfect esteem

your most obed^t serv^t

J. J. Meisner. M.D.
Prof. of Botany at the
University of Bari,
Switzerland.

Mr. Geo. Meiners.

Refused to pay



Chas. Gray

Not a resident

~~Bethel~~

with references



31. Aug. 1842.

My dear Sir,

I ought to begin these lines with a thousand apologies for my long & almost complete silence towards you, but finding that I really have no sufficient excuse for it, I shall rather at once acknowledge myself guilty and with sincere repentance ask your pardon. You would however ~~the~~ do me wrong if you thought that I had neither thought of you nor of the deep obligation in which your kind & most valuable communication of plants & books - which I have received in due time, as well as your letter of March 5th 1844 has placed me, for, indeed since your first letter I have always been thinking of procuring you the desiderata of which it contained a long list, and have employed many of my leisure hours - which alas! are but few & short - in perusing my herbarium for that purpose. But the greater part of your desiderata being of the rarest species ^{of which} - especially those peculiar to Germany - I have either no specimen at all myself or at least no duplicates, I was obliged to apply to other sources for procuring them, and this necessarily caused a long delay. Now at last I have the pleasure of announcing you that two weeks ago I have directed by the "Boulogne" to Messrs. Loranx & Co. Havre, to be forwarded to you by the earliest packet, a case containing the following objects:

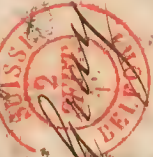
1. A thick parcel of plants for you.
2. Two smaller do. of Cyrenian plants (from Mr Shuttleworth.)
3. A number of brochures on different subjects of Natural History, (together with a few ^{old} prints of the likenesses of Joh. & Casp. Purshin & sons) partly for yourself & Dr Torrey, and partly for the Lyceum of Nat Hist. of New York, to which I would request you to present them in my name.
4. A parcel of plants addressed to Dr Short, Louisville, Kentucky.
5. A parcel of seeds for the same.

I hope it will not occasion you too much trouble to forward the latter parcels to Dr. Hook and that you may easily recover his part of the expenses for freight etc., but in future, if you prefer, I shall direct the parcels destined for him to Philadelphia. The likenesses of the Banksians are all for you and such of your friends as may take an interest in them. I have also added two copies of my "Poedr. monogr. Polyz." which you may give in my name to some of your friends or to some public institutions or scientific societies. My parcel of plants for you contains a good number of your desiderata, with an addition of other Swiss & South European plants and a collection of plants from Rumelia, Macedonia & other parts of ^{European} Turkey, gathered by Trivaldsky. These plants, though frequently in bad specimens, will, I hope, be agreeable to you, as indeed most herbaria have nothing from those parts, and as they will be quoted in Dr. Grisebach's ~~Flora~~ of Rumelia which is now in the press, the author having my own collection in his hands for comparing Trivaldsky's specimens with those he has gathered ^{himself} in the same country. Trivaldsky's plants are frequently quite wrongly named, but these errors will all be corrected by Grisebach. My friend Shuttleworth, a very distinguished botanist and proprietor of a beautiful herbarium & library has requested me to send you, along with my own parcel, a collection of Pyreanean plants to which he has added a letter for yourself and a specimen of *Erythraea spicata* found by Ruzel in the United States, where, I believe, this species, common on the Mediterranean shores, had not yet been found heretofore.

And now let me express you my sincerest & warmest thanks for the highly interesting communications I had the pleasure of receiving from you two years ago. The plants were mostly new to my collection, ^{especially} those from the more western & southern states, particularly Florida, as also the Cyperaceae & Gramineae ^{for which} your & Dr. Torrey's excellent *Flora* of North America possessed already Part I & II, I feel deeply obliged to you, and since you were so kind as to offer to send me the con-

tinuations, I take the liberty of requesting you to send them as soon after their publication ^{as possible} through your bookseller at Paris. I make great use of ~~the~~ first volume for the supplement to my "genera" which I have at last finished just now, and I am very anxious to get acquainted with the following ones. The Composite will give you a good deal of trouble, especially the genera *Aster*, *Solidago* & *Helianthus* the species of which I find almost inextricable such as they are defined in other works.

I have heard with great interest that you have visited last year the higher mountains of North Carolina and discovered a number of new & interesting species, and I need scarcely add that whatever duplicates of them you could spare would be highly gratifying to me. These mountains are, I believe, the chief habitation of the *Waldwandra*, of which I have not yet been able to get any wild specimens. Other genera which I am particularly anxious to get are: *Diphylleia*, *Tonno*, *Veria*, *Mahonia*, *Sarracenia* (I possess only *S. purpurea*) *Magnolia*, *Aralia*, *Panax*, *Smilax*, *Pavia*, *Asarum* & *Heterotropis*, the rarer species of *Aspidistra*, *Peperomia*, *Desmodium*, *Astragalus*, *Robinia*, *Gentiana*, *Potentilla*, *Saxifraga*, *Asclepiades*, *Rhus*, *Quercus*, *Coniferae*, *Melanthaceae*, *Orchideae* & *Ferns*. Authentic representatives of the many new genera of various orders published in your *Flora* would also be of the greatest value to me, - but I almost fear that you will feel little inclined to deal any more with one apparently so slow & negligent in correspondence as I have proved to be till now, but as my great work which for eight years has completely absorbed me & my time is now at an end I can confidently promise that in future my correspondence shall be regular & punctual, and as I always keep in mind your desiderata I hope to be able to send you ere long a new parcel. Meantime, dear Sir, receive once more my warmest thanks and excuses, and believe me ever yours very truly C. D. Miesner.



Dr Asa Gray
Professor of Botany in the University
of Michigan

New York

U. S. of North America.

via Havre.

Churd May 30 —

Basel 28. March 1843.

My dear Sir,

I was very sorry to see by your kind letter of Febr. 1st, which I received eight days ago, that the box I had the pleasure to forward for you to New York last Summer has not yet reached its destination, and I have lost no time in taking such steps as may, I hope, lead to its recovery. I caused the commissioners who sent it from here to M^r. Loraux & Co, Geneva, to write to that place, and I have just received the answer of these gentlemen by which they state that they had duly received my box and shipped it off to New York by the "Ulrica" on the 13th Oct. last. This vessel having since returned to Geneva, there is no doubt of our box being really arrived at New York, but it seems that either by some error in the direction or by negligence of the commissioners at New York it has remained undelivered. Let us hope that by applying through some of your friends to the office where the effects of the Ulrica were deposited at its arrival you will be able to recover it, or at least find out whither it may have gone. Were it lost, I should the more deplore it as part of its contents, especially the Turkish plants, would be almost irreparable. At all events I ask beg you

to favour me as soon as you can with a few lines of information about the result of your inquiries, which I still hope will not remain altogether fruitless. I gave M^r. Loranx & C^o at Havre the direction to Wiley & Putnam, Booksellers, New York, whom you had named me as your commissioners there, and have lately requested them to write to them on purpose to know whether they have received the box and what they have done with it. Anxious as I am that my parcels should at last come to your hands I expect your answer with great impatience, and should it prove unfavourable, I would do my best to repair the loss as soon and as well as possible.

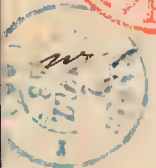
It is not long ago that I have read in several journals the news of your appointment as Professor of Botany at the Harvard University for which I congratulate you with all my heart, thinking that in this situation you will be the more favourably circumstanced for your scientific pursuits, and being fully convinced that your influence as teacher of our favourite science will powerfully contribute to its spreading more and more, through your pupils, all over your vast country and to its being usefully applied by many a rising talent. I am par-

ticularly glad to hear that you have the advantage of possessing a Botanic Garden and it would give me great pleasure to exchange with you seeds, as I do every year with about 25 of the chief Gardens of Germany, Russia, Italy, France & Holland, so that I am able to supply you with a great variety of plants of all countries, and especially of the new species which every year appear in the catalogues.

Many thanks for your kind offer of the continuations of your excellent Flora, of which I have not yet seen more than the first volume. Small parcels will ^{reach} ~~send~~ me best through Mr W^m Pamplin ^{(Queen Street, Soho Sq^r),} London, who frequently sends things to my friend Shuttleworth, and with whom I dare say you are also in connexion.

Believe me, dear Sir, with sincere esteem
yours very truly

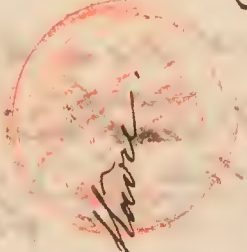
J. P. Meisner.



Dr. Asa Gray



Professor of Botany at Harvard University



Cambridge

Massachusetts

U.S.A.

*Will of Carding only can
in the last but if you can
Care enough directed*



Nasel Sept. 3. 1869.

My dear Sir,

My health is at present tolerable. I have spent three weeks at the Leuk in the valley of the Simme. Canton Bern, for the sake of rest & good air, but the place is not elevated enough (about 3000' only over the level of the Sea) to afford true invigorating mountain air, and even there we suffered greatly from the uncommon heat in July. The scenery is very fine, but little known to strangers, and the surrounding mountain & glaciers, reaching the height of 11,000 feet, are good botanizing ground, but my short breath & general weakness did not allow me to visit the higher & more promising spots, so I returned with only an indifferent harvest. Having finished the *Convolvulaceae* for the *Flores Brasiliensis* (they were even published lately) I have now work under hands for the present, but occupy myself merely with what I may call quiet & private studies in my herbarium, and therefore I am still very glad & thankful if I receive any accipions to it.

But now God bye! I wish & hope you will have a good passage to America and long enjoy the circumstances of your late voyage & travels to the Old World, in good health & happiness. God be with you!

Ever yours with sincere esteem & regard

L. F. Meisner Prof. emer.

D. Asa Gray.

It seems to be written in Heaven that we shall never meet in this life! All my wishes & hopes of at last seeing you during your late visit to Switzerland were vain, and I dare not hope now that if you return once more I shall have better luck, for then very likely I shall be no longer among the living. Twice already, years ago, you have been here when I was absent, and this time I did all I could to avoid missing you again. Taking it as a matter of course that at all events you would go to Geneva, I repeatedly wrote to our friend DeCandolle to let him know, for your sake, where I should be to be found at the supposed time of your stay in Switzerland, and he was kind enough to inform me of your movements, telling me, the last time, that you had left Geneva already for Interlaken & Luzerne, but without mentioning till when I might find you at one or the other of these places, and adding that you intended returning to England in August for the British Association at Exeter. When I received this information (on Aug. 3^d) it was too late & too uncertain to run after you with any hope of success, yet when I came to Bern a few days afterwards I still had some hope to find you perhaps at our friends Shuttleworths, but in vain, and I learnt from him that you were then already in England. I cannot tell you how much I am vexed & grieved, the wish & hope of seeing you & of thanking you *viva voce* for all the kindness you have always shown me, having so long been - and shall still be to my last - foremost & one of the dearest of my heart.

Presuming that you are probably still in England, I direct this letter to the care of Dr Hooker, Kew, whence it will no doubt be forwarded to you, wherever you may be, even should you already have sailed for America. I send you also a copy of my memoir on the life & writings of Martins, written on request of the Bavarian Academy. I take the liberty to add another copy, with the request to present it in my name to the Boston Academy of Sciences & Arts.

I am still expecting the result of my attempts to enter into connexion with some American Botanists from whom I might receive the rarer plants of the U. S., especially from the more Western parts, and from the Sandwich Islands. According to your recommendation I have written, in June last, to Mr Horace Mann, with a rather great list of desiderata, but I have not yet received an answer, perhaps, I fear, on account of his health. I have not written to Dr C. C. Parry, Davenport, Iowa, thinking it almost superfluous since you have sent him my letter, but I think now I had better do it, unless I receive a letter from him in a short time. From Bolander I had a letter this spring in which he promises to send me shortly a parcel, together with one for Mr DeCandolle, which, however, I have not yet received. ~ I am extremely glad to hear from Mr DeCandolle that you intend to continue your excellent "Flora of N. Am.", though on a somewhat altered plan, more similar to your "Manual" which I am sorry to say I have not yet seen, no more than Chapman's Flora of the Southern U. S. We cannot get here such works, except on special order, but I hope I may procure them from England. I am most happy to hear also that your trip to Egypt has proved perfectly successful & agreeable, and that Mr Gray has so well recovered his health. I hope & wish with all my heart that it will be lasting for ever.

You will be surprised to hear that the Herbarium of Martins will not be purchased by the Bavarian Government. Dr Eichler just writes me that Prof. Liebig & Nageli who had been charged by the Minister to report & advise on the subject, had at last done it, but in such an indifferent and almost disdainful manner that the Minister will not even bring the matter before the Chamber of Deputies. Is not it a pity & a shame? the more so, as the public Herbarium of Munich ("Herb. Acad. Monac.") is not at all a rich one, and greatly inferior to the other Museums (for Zoology, Anatomy, Paleontology, Mineralogy etc.) of that city, - its only valuable parts being the Brazilian plants collected by Martins, Bohl & a few others, the Herb. of Schreber & some other small collections, - while by the acquisition of Martins' collections, it would have been brought to a more uniform & universal completion and raised to equal rank with the other public collections of Munich, and even almost with the Royal & Imperial Herbaria of Berlin, Vienna & St Petersburg. It will be rather difficult now to find a purchaser, especially in Germany, where Governments have at present no money except for soldiers & guns, and where - thanks to the reigning school of ^{the} Microscopists - the interest for systematic Botany & collections has sunk to a very low ebb. Indeed, I expect that sometime hence nobody in Germany will have a knowledge of species, - much less of the vegetable kingdom in general! If I should give ask an Architect or an Artist to give me a description of Westminster Abbey for instance, and he gave me the most minute account of every single stone & all the other building materials that have been employed in the grand Edifice, - but nothing more - would that give me a real idea or image of the whole building & make me acquainted with the laws & beauties & peculiarities of its architectural style? Why, just that is the case with our modern Professors & teachers of Botany in Germany. With a supreme disdain for the true Botany, i.e. the knowledge of plants & the distinction of species etc., they confine themselves to the more anatomical studies and do not even know the name of the commonest plant that grows at their house door!

To Prof. Asa Gray.

Basel Febr. 17th 1872.

My dear Sir,

I have just received with unutterable pleasure & thanks your & Dr Torrey's most welcome & kind letter, and I hope you will excuse me that, instead of replying to you in extenso, I send you my enclosed answer to Dr Torrey with a note to Mr Crooke, both which I desire you to read before forwarding them to New York, the exact adreps of these friends being unknown to me. Thus you will see what I think & feel concerning Mr Crooke's intentions & liberal offers which he makes to me in such a noble, delicate & sympathizing manner, for, what I have written to Dr Torrey, I would exactly have said to you too, and so one letter may perfectly serve for two. I need not, therefore, repeat here that I gladly accept all offers & proposals made to me by Mr Crooke & yourself, nor how happy I am that Mr C. will kindly allow me to retain still for some time certain parts of my Herb., a proposal which greatly soothes & smooths the painful separation from such an old friend & daily companion. I look forward on this final & total separation about as a tender father would look on his beloved daughter's going to be married far away, when all his feelings will concentrate themselves in the wish & prayer that she be united in happiness to a worthy, noble minded & loving husband, — but Mr Crooke's intentions, particularly that of placing my Herb. in the Columbia College, New York, leave me no further wish, as to the future lot of ^{this} my dear child!

I believe I have already told you in a former letter that I shall go on collecting till my last days, - therefore, if you can engage any of your friends to send me plants, for inst. from the western States, California, Sandwich Islands etc., I would receive them with great pleasure & still incorporate them in my Herb.

I hope & trust you will receive as quickly as possible Alph. De Candolle's & Shuttleworth's report on my herb., and I need not say that I am always at your service for any information concerning it you may wish to receive.

You say nothing of Mr Gray's & your own health; so let me hope that I may take it as a good sign. My own health, I am happy & thankful to say, is much improved since several months, so that I am able again to ^{be up &} occupy myself the whole day and dare look with better confidence on the future. Poor Lenormand's death (^{Dec.} Dec. 11. 1871.) was a hard blow to me, he was in every respect a most excellent, worthy man, and the most agreeable correspondent that one could wish.

With kindest regards to Mr Gray believe me,
my dear Sir, ever your truly attached & obliged
friend

J. F. Meissner.

Basel, Switzerland
Febr. 11. 1873.

My dear Sir,

it is so very very long that we have exchanged our last letters that I could not wonder nor complain, if you had meanwhile given me up as a correspondent! Indeed it was in July 1872 that I received your & Dr Torrey's last letter, and at the beginning of Sept. that I sent you my answer. You had then both written to me just before you were on the point of starting together for your great journey to California, Oregon & Mount Gray, and you kindly promised to give me some account of it after your return, which would have interested me in the highest degree, but which I have not yet received. I hope & trust however that your journey has been happily accomplished, and that you are long since returned home, well satisfied with your journey. In my last I have already expressed to you & Dr Torrey my warmest thanks for the kind ^{real} ^{with} which you have so successfully promoted the sale of my Herbs to Mr Crooke, New York, and now I hoped to be able to inform you of the safe arrival of part of it in New York, of which however, I am surprised & sorry to say I am myself still ignorant. At present I can only tell you that about ~~two~~ ^{three} thirds of my whole Herbs, consisting of eight very large & strong boxes, carefully packed ^{by a friend} and accompanied with all the necessary papers procured by our American Consul, Mr Erni, have been shipped at Rotterdam in an American Steam packet which must have arrived at New York long ago. Now you may imagine how anxiously I wish to be made sure of its safe arrival, and I should be most thankful to you for procuring me any information concerning it. Though the season & weather

were not the most favourable at the time, yet we have no particular ^{reason for} fear of the loss of our vessel & cargo, at least neither our commissioner nor the Consul who have written both to Rotterdam & to New York for information, have heard nothing of our vessel being among the number of the losses at sea that took place about that time. We still hope therefore to hear soon of ^{its} safe arrival at New York, and also of the ^{transmission} ~~payment~~ of the rest of the payment of the whole (5000 Doll.) which according to Dr Torrey's last letter, had already been put at his disposal, although Mr Crook did not press me at all to accelerate the transmission of the Herb. On the contrary he has dealt with me from the beginning in the most noble & generous manner, expressing quite spontaneously his wish that I would keep the Herb. with me as long as I might wish & think to make use of it. I have therefore never ceased to collect & to enrich it & to improve its arrangement, it being my pride & my duty to do so.

In the precarious state of my health which obliges me to pass a great deal of my time in bed, I can of course do but very little and am almost

out of connexion with the botanical world & unable to give you scientific news & must cut this letter rather abruptly. I dare hope however to get much better & stronger in the good season & still to be favoured still with your & Dr Torrey's letters. Meanwhile believe me, dear Sir, ever yours faithfully
L. F. Weisner.

P. S., the packing up & forwarding my Herb. has been done by the great kindness of Dr Aitken, a very distinguished botanist of Basel, and he has also written on the subject to the Columbia College, New York.

P. 5 -

A few berries of
Vacc. Arborescens (Fertile berry Bluffton
only Fuschberg) are in
the top of the box
gathered in winter of '70.
I regret that this box is so small & so
too badly packed - but the best
I could do.

Sturges Parrot

Socla

22 Nov 71

My dear -

My dear,

Your note is referred
to the C. Hystrix I send in due
time & as an opportunity offered
this morning for a further
search I was fortunate enough
to find two compact clusters of
the berries. These were mostly
quite green & had not yet assumed
the yellowish cast of maturity & ex-
cept in a few instances; I
doubt not however that they are
ripe & were only awaiting one
or two more frosts to deepen their
colour. I did find no sufficient
small winter plants of the same,
although I must have examined

The old

Send 20 or 30 specimens A-I however
I dug up one sufficiently small which
seemed to be growing from the main
subterranean stem of another (proli-
ferous?). At any rate I was compelled
to sever what seemed the connection, but
as I immediately packed it close in its
native earth or swamp mud
I think it may "live". Most of
these plants of *C. Hystrix* were erect
growing alone, with a trunk of
about 18' ^{to 20'} or thereabouts, - but others
again were clustered as if runners
or subterranean stems had been pushed
out from the main tree. This
is certainly not the case in ^{either} the
large, or dwarf Palmetto, but I sus-
pect ~~the~~ ^{may be} the case with the Sam
Palmetto (*S. Serot.*). However I
do not know - never having examined!
I sent a box or two direct of Xpress
from Savannah Ga with the above rooted
C. Hystrix, also the drupe of this & *Sabal*
Adami & *S. Serot.*. I have or
yet seen none on the large Palmetto, tho'
they live abundantly elsewhere if I
mistake not. No true cane-bugs
near here there about the South river some
20 miles off -
x none cultivated!

When this note is
read you will know
that the box may sent by Xpress at any time.

J. H. Mellichamp

wrote me also, that you
had not replied to him,
& wanted to know what
had become of you -
as I believe he had
expected you there.

I have been under
the weather during the
whole fall, & greatly trou-
-bled with a kind of
Autumnal - or as I prefer
calling it - malarial - Catarrh,
which answers in all respects
I suppose to yr Hay fever
or Hay Asthma at the North.
Its periodicity with me for
the past 12 years is remarkable.

I am here therefore for
health's sake.

V. H. M.

Charleston
S. C.
8 Nov '75.

To -

Prof. A. Gray.
My Dear Sir

Some years back
you got me to send you
for Mr. Cairne the fruit
of C. hystrix. Just be-
-fore leaving home hop-
-ing to meet with
several plants in a
new spot - I secured a
few of the "heads" - one
of which I now send you.

I do not know if
they are perfectly mature,
- but those which you will
find enclosed in a

little bag certainly are,
as they are last year's
seed & were found in
among the spines.

There are certainly sound,
- some of them having drop-
- ped in the mud & leaves
germinated, & one of such
is also sent.

I have at home
three or four stout young
plants of the same in
no way mutilated & in
perfect condition. They
are too bulky to be
sent through the mail
but they might be very
easily sent by express.

Should you care
to have them they

maybe sent you
in that way.

Very respectfully,
- A.S. - J. H. Mearns

I trust my dear
your health is now
quite restored. After
reading and answering
your note recd at Hardee-
ville I never heard from
you again, & only heard
through other sources (Mr
Cauly) that your trip
had benefited you.

My relative W.
Cromwell of Albany &c.

& we almost despair
 of getting the moral
 support of good people
 who ought to know us
 better. All these things
 are untrue ^{which} are
 told of us & it is a
 concerted movement for
 political effect of that
 government which has
 help us to law & order
 & preserve our property
 from confiscation! Think
 of me my 25, 23, with 7th
 as a firm manager of Election
 placed between a Northern
 Carpet bagger & a Negro militiaman
 Captain - I watching them
 so as to give a fair count
 for Wade Hampton! Laugh
 or weep as may seem to you
 fit! I count this a "tremen-
 dous" effort on my part for
 the State - ~~the State~~
~~the State~~ & may God bless
 the humblest efforts of all

his son, Arthur
 & I expect
 will have written

these - it is not a blood of honor -
 hearts are full. But you

but one

Nov 2. yesterday my ear was dimmed with the roar of the Republic
 - near water (Black Bull of Bashan) under the live oaks near my house &

11/27/76 - I wish to see you & things I bring with me
 - Bluff Nov 76
 1. Nov 76 -
 Frankfort Gray
 You heard need last night
 was like a gleam of sun-
 - shine or shall I say a
 messenger of Peace to one
 living in this unhappy &
 distracted State! But
 you she has written a
 month or two ago, for although
 I have looked for Podostegma
 in a spot where I have
 frequently seen it. I was un-
 - able to find even the
 plants. I have often seen
 the pods, & I may get
 them yet - if so I shall
 send on immediately. But
 I must send you something
 so I mail with this a
 piece containing sps of
Aclepias obovata with
 pod - (or is it Tomentosa?)
 and also of Scutellaria

under such inspiration I wrote last night

mantima (Spain - EM) &
 this is the nearest I can
 come to your Express
 with! But you may
 get the Podostema roots
 yet! Did you get the
 piece of Hydnorales as
 with letter sent in the
 summer? I thought they
 would be acceptable after
 Dr. Engelmann's oaks
 came out.

Things are very
 chaotic in Carolina,
 & great apprehensions
 are expressed here on
 the sea-board - (especially
 since the Cain boy
 massacre of whites,) and
 I regret to say that
 some of our women are
 even moving off - preferring

the worst, but the most emotional - it is only when money & power
 are very kind - they are my best friends & give me the bread
 I eat for the most part - all of us here get on well with them.

to face yellow fever in
 the neighbourhood of Savannah
 rather than remain in
 Ashantee! My family
 will stick. The up-
 -country swarms with
 U.S. troops, while here
 on the sea-board where
 they would really be of
 service in preserving
 order & preventing the
 mal-treatment & intimid-
 ation of colored Democrats
 not one is to be found.
 We are much pained
 that we shall be so
 much misrepresented
 at the North in this
 our last (?) effort to
 overthrow the
 rule of Thieves & brigands

* Of course I saw neither leaves or flowers - it being fall or winter -
 that I had seen two species of gonolobus here, one with spiny fruit & the other smooth or ribbed, - but that I had not seen the last for a long time". That was a little too hasty & inaccurate, but evidently I had in my mind a single bursting pod wh I found at "Pritchard's* plantation" in 1860 - '61, and wh I believe was smooth or ribbed. But that was a long time ago, and I now do not feel certain about it! I hope yet to find that same plant & to find also smooth fruit, for Dr. E - had a right to ask as to the discrepant statements. J. H. M.
 only the brown pod.

15th Parcel of oaks last week with letter sent. A hybrid of slute thunk of D. laurifolia & calostoma!
 Bluffton
 So. Ca
 6. Dec. 1876.
 To
 Prof Gray,
 My Dear Dr,
 Your gonolobus card came last night. Since returning here in '67 I remember to have examined only three diff. vines of gonolobus - viz, one at Bluffton, one at Sandy Run, and one on the Okechee, - from all three of wh I have collected much fruit, and from the ~~two~~

first two of which only
did I obtain flowering
sp. The large pods
last sent you came from
Sandy Run, and as I
got no flowers there this
year or the last - I
conclude that those you
refer to and describe
came from that spot.

From time to time
I have seen plants
on the road side, but
I do not remember
that I ever examined
them. Next year I
hope to clear up
the mystery, and

it seems not unlikely
that the flowers
with "corolla whitish
pubescent inside - smooth
outside" - will be found
on vines bearing prickly
fruit at Sandy Run !!

Please keep this
note till next summer
when I send sps.

Very truly
& respectfully

J. H. Menckamp

P.S. - Dr. Engelmann
has written me like-
-wise, and says that
in '74 I wrote him

not sure) might belong to the
 sps bearing Smooth & ribbed fruit.
 My seeds & collections were all
 scattered by our troops occupying
 my house, & I can't but think
 that these vines (found only in my
 old lot) came from the ant bed
 seed - what I had expected to
 germinate under the fostering
 care of Mr. Cairne in the
 Jardin des Plantes! And so
 I think I was right after all
 in saying that I had seen
 the smooth & 5 ribbed follicle!

Very truly & respectfully

N. S. J. H. Mearns

Last week I sent you a packet containing
 the leaves of S. variolans leaves, & a few days
 before, a letter, containing a repetition of my ex-
 periments with the sweet secretion. I have since
 tried the same expts as in '74 with nests of
 the large ant, by throwing in the portions of
 leaf viscid & sweet. The result was just as
 before. The ants were ravenous, & after watching
 them a long time I left them tugging to pull
 the morsels into their holes, but in no single
 instance was there any sign of unsteadiness or
 stupefaction or intoxication. As ants are especially
 fond of the Sac. honey & are among the very first
 captives, - one would have expected to have seen them
 quickly stupefied if the secretion were indeed
 toxic, - but not one showed it! The other day
 I watched several insects feeding up & down the
 trail, but no effect was produced on them. I say of

Petals of this S. macrophylla are humples
 at base, with ^{greenish} yellow tips. Bluffton ^{or greenish} hairy within.
 I enclose also S. sub ^{fls of S -}
^{hirsutus} just collected, - both ^{humples} ^{or yellow}
^{greenish} yellow & ... 6 June. '78) the inter-

Trifolium - varying shades.
 This fall will
 send you the fol-
 lowing of S. M.

I have just found
 the Gonolobus of which you
 wrote me last fall! no
 doubt it is Chapman's S -
macrophylla - and yes
S - suberosus. I enclose
 a few flowers & a leaf
 from the same vine, and
 have made a rough
 sketch both of the dried
5-ribbed smooth follicle
of last year, and also
 of a single flower with
 its recurved petals.

~~Several~~ Several vines
 were found all growing

near each other, but
the dried follicles of last yr,
and the flowers & leaf
enclosed did not come
from the same vine. There
were no flowers as yet on
the vine with the follicle.
The latter I shall send to
day to Dr Engelmann - as
also a few flowers. —

I had been hunting
for this thing wherever I
went. Ever since I wrote
you, and had almost
given up the search,
when this morning
happening to walk
through my old burned
lot - now all grown
up, - I was attracted by
a young vine - the leaves
of which struck me as being
more delicate & white veined

than ^{the} hirsutius I sent you
a short time back! I was
satisfied I was on the
track at last, and after
hunting a little - soon "struck"
the mother vine, which was just
blooming, - twining around
a young pine sapling which
had shot up near the
crumbling bricks of my
old kitchen chimney!
(None in bloom but this
one. —)

It is very likely that these
vines (which I've found no where
else about here) came from
seed which I had collected
just before the war! I think
I mentioned to you - that to
oblige a friend who has
promised some of our native
seed to Mr. Caine - I had
collected follicles of a
gnatopus ~~2~~ at a certain
place, which at this distance
of time I believed (but was

If you thought it worth while
to preserve that Asclep^d, fol=
=icle I sent you last winter
you can place the enclosed
slender shoot alongside of
it, as both came from the
same root - which I trans=
=planted. It has just shot
up that, and without a
doubt it is Anantherix
cornivorus. The parcel
contains also two follicles
of Podostigma pubescens
- wh^{ch} I believe you asked
me for some four years
ago. If the Engeron
turns out to be E. quercifolium
=folium I'd like to
know! Wishing you
a pleasant voyage and
all manner of happiness -
I am my Dear Doctor
Very sincerely yours
J. H. Mellichamp

Mellichamp
Bluff Mt C

31. July 1880.

To
L. Asa Gray -
My Dear Doctor -
Last week I
sent you a letter
enclosing a few more
plants of J. Jalapa,
and also thanking
you for the nice
book wh^{ch} came
safely. On the ^{30th}~~26th~~
I rec^d your card in
reference to Engeron
quercifolium. Never
having had my at=
=tention called to

this genus. I have
hardly examined them -
and don't know whether
E. quercifolium is pe-
= rennial, - or stolon =
= iferous, or not! I
think I have seen
here E. striosum &
E. Canadense - but
am not sure. Every-
= thing has been
dried up by the
long drought, but I
have just found in
a low corn-field
(now almost a bog
from the recent rains)
a few poor specimens

of what may be E.
quercifolium, - but I
am such a poor hand
at the Composite
that I cannot venture
to say! I suspect from
the looks of the root
of one of the sps that
it does send up off =
= shoots, and it may
be perennial. I think
it is. I send you with
this what I found,
and will now continue
to look - and I may be
able to send other and
better ones by the
time you get back
from Europe, - or
even before Sep.!

Bluffton

So-la

14.55c.85.

To W. Asa Gray -

My Dear Doctor -

It is late, - but
not too late I hope - for
me to join in the con-
=gratulations of those friends
of yours who saluted you
on the occasion of your
last birthday!

Of all of these well-
=wishers there is no one who
thinks of you with more
affectionate regard than I
do, - or who with more
sincere wishes you
many years yet of labour
and happiness!

I am Dear Doctor

Very truly & respectfully
Yrs J. H. Mulvickham

no trace of the clusters
of miniature ^{and young} leaves, — and
one small plant which
has a good many.

This is the only condition
I have seen ^{at approaches}
what you refer to.

Pogonia and Amianthum
growing near were thrust
in the parcel, and
also pulped spurs of
Asclepias michxii, Decain,
wh I found last week
a long distance off,
— also Podostemum pub

ell. Sorry to be so
long in writing!

I am Dr. Doctor
Yours very truly,

J. H. Mellichamp

15 I have not yet recd
~~at the~~ the article
Bluffton sent.

S. C.

25 May 86.

To
H. C. Gray,

Dr. Doctor,

Yr. postal
in reference to S. flava =
= cenia I recd last night.

S. flava is not found
here on salt water, so
that I can tell you
nothing of its be =
= harious. S. Variolans
has another set of
leaves appearing sparsely
later in the year which
are well-grown.

These secrete abundantly,
and capture insects even
more largely than those
in May & June - till
cold weather. As for
the small phyllodia
you refer to in S. flava
I have never seen such
just as you describe
in S. variolans, but I
do see here and there
a few plants with
clusters of miniature,
perfect, & inflated leaves
growing at the bases of
the old & large leaves.

These are two or three
inches high; whether
when they first emerge
they are flat and claw

I do not know (I suppose
they are), but as usually seen
when still very small they are
hollow and perfect in shape,
thus 

They come to nothing.
One does not see them
especially in Autumn - but
at any time of the Summer
it seems to me. I saw
two or three clusters today
but most old plants I
think do not show
them at all, - rather
the younger ones, but I
have seen them on what
appeared to be old plants.

With this I send you
a parcel put up roughly
in the wood, - (or partly
so) which will show
you old plants with

Bluffton

S. C.

24. Aug. 87.

My Dear Doctor Gray.

My friend Mr. Kennell Smith
has but lately informed me
of your having named a new
genus for me, — he having
kindly transcribed the dedication.

It was a very great
surprise to me but a most
agreeable one, — especially
as indicating a continuance
of that friendly regard which
you have so long shown me,
and which I value so much!

I thank you my Dear Doctor,
and am your
much obliged

J. H. Mearns

To
L. C. Gray
Cambridge
Mass

over —

P.S.

If I mistake not you
said somewhere that fruit
of Asimina parviflora
was wanted - I send some
collected yesterday in a
sandy pine land, and
them in the hard mature
fruit of Schizandra wh
more of service in
exchanging.

J.D.M.

could be - was - awfully
 severe - rigid - stern and
 Rhodamantean! Witness
 what he writes about that
 splendid man a gentleman
 - our President, - and
 again how much on board
 the wonderful man he
 wrote about - the Hon
 Zephloroch (?) whom in
 the old College days we
 used to worship so! But
 our Dr. Boeton was no
 half way man, & he lived
 the union & was ready to
 die for it, - but that's all
 passed and gone now! I
 can't imagine a man's passing
 a happier life, - nobler & truer
 than Dr. Boeton passed! I need go
 last letter. Thanks! Yes! I had
 seen those lines of Swells' and thought
 them absolutely wonderful in their
 sweetness & truth & perfection.
 Only a true poet could have written
 them - how they must have cheered the
 doctor's loving heart. Please don't take

Bagg
 21. Nov 1894

My Dear Mr. Canby
 I feel I'd better
 send in these Sprays
 of Pilotum tie =
 = Spectrum at once,
 though I don't know
 whether you care
 for them. I think
 I sent you one a
 good many years ago.
 It seems extremely
 rare - never saw
 it anywhere but
 here - two or three

growing under live
oaks. They seemed to
disappear, - but finding
these - and as the hogs
were rooting around
them I took them
up to save them (if
of any value) for
Herb Shrimps. I
never saw but one
in fruit, and this
was a single plant
growing under a
pine-sapling in the
pine-trash! I'm
through now with
Dr. Gray's life and letters

- don't know that I ever
enjoyed a book more
in my life, - what a
perfect life! - To his,
besides, a numerous - and
always troops of loving
friends greeting him
everywhere he went.
And so well did
he deserve all of
this love - for he had
a loving and tender
heart, - yet oh my!
he could hate too -
(no! I won't say
that!) but in the
ally of duty or what
he considered such
- to his Country - he

À M^{rs}. Anna Gray Secrétaire de l'Académie Américaine, des
Sciences et des Arts de Boston.

Monsieur le Secrétaire !

La personne qui vous présentera avec cette lettre, le prem.^{er} vol.
de mon Thermochroite, et mes deux derniers exemplaires des mémoires
sur le Magnétisme des rochers que j'ai lu dernièrement à l'Académie
des sciences, de Cayler, est M^r. Bendolari pianiste très-
distingué et excellent professeur de musique, qui part d'ici avec l'in-
tention de s'établir chez vous. Muni d'excellentes recommandations
politiques et financières, celle d'un simple académicien lui aurait été
parce superflue sans aucun doute, si tel lien d'amitié qui nous u-
nissent depuis longtemps ne l'avaient persuadé du contraire. Vous
êtes, Monsieur et honorable confrère, mon seul correspondant à Bos-
ton... il faut donc que je vous prie instamment de lui procurer les re-
lations qui pourront lui être, le plus avantageuses sans cette capitale.

Outre son habileté bien connue M^r. Bendolari est un jeune
homme, extrêmement honnête et fort aimable en société : ses mœurs sont
irreprochables... il part regretté, bien regretté, de ses amis, et connaît bien
ce que vous serez très reconnaissant de tout ce que vous ferez à son avantage,
et vous pouvez compter franchement sur moi dans le cas où il s'agirait
de rendre ici les mêmes services à l'un de vos recommandés.

Je vous prie en attendant recevoir d'avance mes plus vifs remerci-
ments et me croire tel que je suis, effectivement.

Anna Gray Secrétaire.

Cayler le 15 Février 1854.

Votre très-humble et très-obligé
Lecteur et collègue,
M^r. Bendolari & M^r. Melloni

Monsieur
Monsieur Ada Gray
Secrétaire de l'Académie Américaine
des Sciences et des Arts de
Boston,
(Massachusetts.)

L. Menand

Menand Station,

Albany, N.Y. - Apt 25 1889

Swano Watson Esq.

Dear Sir

Would you have
the kindness to tell me the names of
the two Compositae enclosed. I have received
them from Colorado salt springs, one
grows 3 feet high the yellow one, the blue one
about one foot

Sincerely yours

L. Menand.

My Dear Ward,

I shall be very happy
to receive you & Dr Gray to
dinner on Saturday next—come
as early as you can and if you
bring Mrs Ward with you it
will greatly add to Miss Brown's
pleasure & mine—I remain

Yours most truly
S. Newries

Ladbroke Terrace

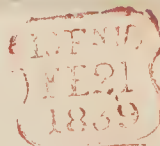
20th Feb'y - 38

(9)

N. B. Ward Esq:

Longdon

Well-close Square



Hammer-smith

My dear Sir 6 June 1844

I am much obliged for
your note apprizing me that you
have received some plants which
Mr. Harvey has kindly sent for me
and that no doubt I shall prize my
highly. I shall be engaged all the
beginning of the week and cannot
have the pleasure of calling on you
before Thursday next. When I will
do so on my return from Ratchiff
about 2 — and then I shall hope to
find Mr. Ward quite recovered.

Mr. Miers desires to write not me
in kind regards to her - yourself - &
all your family circle -

Love my dear Sir

Yours very truly

John Miers

Temple Lodge Hamworth
W. London - July 1852.

My dear Doct. Gray

In putting away my papers preparatory to my usual trip on the Continent I have discovered to my great confusion a letter I had addressed to you in the beginning of March last which I thought had been forwarded to you at that time. I must therefore now beg your indulgence for this careleft oversight and hope you will excuse such seeming want of attention. That letter was written in reply to your very kind favour of the 26 Dec^r last thanking you for your remembrance and for the information of a *Menispermaceous* plant you enclosed which I examined and found not to be a *Stephania* as you concluded, but to agree with *Glypea* as I defined it. It will be interesting to know whether this species you have from Tahiti accords with Forster's original plant the locality of which is not recorded. For this reason you will oblige me if you can at your leisure trace on a bit of this paper, merely to give a rough outline of the leaf and general habit of the plant, with such particulars on its pubescence &c. as you may think necessary. I have lately been much engaged with the *Menispermaceae*, as M. Defandolles and M. de Boissier kindly sent me the plants of this family out of their herbaria for me to determine, and I gladly availed myself of the presence of the plants before me to work out many details that I have not hitherto been able to do when I have to examine specimens at a distance from home.

You conclude rightly that in my paper on *Menisperm.* *Limacia Petala* 0

is a typographical error for Petalob. I long ago determined Cuming's
n° 2402 to be Limacia velutina. You enquire if I know of any
Australasian Stephania - there is one in Allan Cunningham's collection
I have called S. australis, and also a Clypeus from Port Jackson in Mr.
Defandoller's herb. - collected by Gaudichaud. There is also a plant from
Mr. Arthur in Sir W. Hooker's hb. with 5 flowers only, which I take to be a species
of Diplosclisia and another of S. Cunninghamii without flowers or fruit in the
flora of that continent appears extremely scant in plants of this family
which exist so abundantly in the neighbouring Archipelago and the Indian
Peninsula. When I was at West Park Doct. Hooker gave me Part 2 of your
Plantae Lindheianae which you kindly sent me and a few days ago
I found at the same loc. Part 1. of your Plantae Wrightianae which you
did me the favour to direct to me. I beg to return you my very
best thanks for these useful presents which I esteem very highly.

On my return home I must apply myself seriously to bring up the long
pending arrears and finish what I have partly done towards the completion
of the 2nd Pl. of my Illustrations which shall be forwarded to you as soon as
finished: after that I shall get on with the contributions now
so much in arrears. Mr. Miers and my daughter unite with
me in very kind regards to Mrs. Gray and to you and shall be glad to
hear you have both been in this enjoyment of good health. I remain
Believe me to be very dear Sir, your truly friend
John Miers

Ms. A. 9. 2. 10. 11. 12. 13. 14. 15. 16. 17. 18. 19. 20. 21. 22. 23. 24. 25. 26. 27. 28. 29. 30. 31. 32. 33. 34. 35. 36. 37. 38. 39. 40. 41. 42. 43. 44. 45. 46. 47. 48. 49. 50. 51. 52. 53. 54. 55. 56. 57. 58. 59. 60. 61. 62. 63. 64. 65. 66. 67. 68. 69. 70. 71. 72. 73. 74. 75. 76. 77. 78. 79. 80. 81. 82. 83. 84. 85. 86. 87. 88. 89. 90. 91. 92. 93. 94. 95. 96. 97. 98. 99. 100. 101. 102. 103. 104. 105. 106. 107. 108. 109. 110. 111. 112. 113. 114. 115. 116. 117. 118. 119. 120. 121. 122. 123. 124. 125. 126. 127. 128. 129. 130. 131. 132. 133. 134. 135. 136. 137. 138. 139. 140. 141. 142. 143. 144. 145. 146. 147. 148. 149. 150. 151. 152. 153. 154. 155. 156. 157. 158. 159. 160. 161. 162. 163. 164. 165. 166. 167. 168. 169. 170. 171. 172. 173. 174. 175. 176. 177. 178. 179. 180. 181. 182. 183. 184. 185. 186. 187. 188. 189. 190. 191. 192. 193. 194. 195. 196. 197. 198. 199. 200. 201. 202. 203. 204. 205. 206. 207. 208. 209. 210. 211. 212. 213. 214. 215. 216. 217. 218. 219. 220. 221. 222. 223. 224. 225. 226. 227. 228. 229. 230. 231. 232. 233. 234. 235. 236. 237. 238. 239. 240. 241. 242. 243. 244. 245. 246. 247. 248. 249. 250. 251. 252. 253. 254. 255. 256. 257. 258. 259. 260. 261. 262. 263. 264. 265. 266. 267. 268. 269. 270. 271. 272. 273. 274. 275. 276. 277. 278. 279. 280. 281. 282. 283. 284. 285. 286. 287. 288. 289. 290. 291. 292. 293. 294. 295. 296. 297. 298. 299. 300. 301. 302. 303. 304. 305. 306. 307. 308. 309. 310. 311. 312. 313. 314. 315. 316. 317. 318. 319. 320. 321. 322. 323. 324. 325. 326. 327. 328. 329. 330. 331. 332. 333. 334. 335. 336. 337. 338. 339. 340. 341. 342. 343. 344. 345. 346. 347. 348. 349. 350. 351. 352. 353. 354. 355. 356. 357. 358. 359. 360. 361. 362. 363. 364. 365. 366. 367. 368. 369. 370. 371. 372. 373. 374. 375. 376. 377. 378. 379. 380. 381. 382. 383. 384. 385. 386. 387. 388. 389. 390. 391. 392. 393. 394. 395. 396. 397. 398. 399. 400. 401. 402. 403. 404. 405. 406. 407. 408. 409. 410. 411. 412. 413. 414. 415. 416. 417. 418. 419. 420. 421. 422. 423. 424. 425. 426. 427. 428. 429. 430. 431. 432. 433. 434. 435. 436. 437. 438. 439. 440. 441. 442. 443. 444. 445. 446. 447. 448. 449. 450. 451. 452. 453. 454. 455. 456. 457. 458. 459. 460. 461. 462. 463. 464. 465. 466. 467. 468. 469. 470. 471. 472. 473. 474. 475. 476. 477. 478. 479. 480. 481. 482. 483. 484. 485. 486. 487. 488. 489. 490. 491. 492. 493. 494. 495. 496. 497. 498. 499. 500. 501. 502. 503. 504. 505. 506. 507. 508. 509. 510. 511. 512. 513. 514. 515. 516. 517. 518. 519. 520. 521. 522. 523. 524. 525. 526. 527. 528. 529. 530. 531. 532. 533. 534. 535. 536. 537. 538. 539. 540. 541. 542. 543. 544. 545. 546. 547. 548. 549. 550. 551. 552. 553. 554. 555. 556. 557. 558. 559. 560. 561. 562. 563. 564. 565. 566. 567. 568. 569. 570. 571. 572. 573. 574. 575. 576. 577. 578. 579. 580. 581. 582. 583. 584. 585. 586. 587. 588. 589. 590. 591. 592. 593. 594. 595. 596. 597. 598. 599. 600. 601. 602. 603. 604. 605. 606. 607. 608. 609. 610. 611. 612. 613. 614. 615. 616. 617. 618. 619. 620. 621. 622. 623. 624. 625. 626. 627. 628. 629. 630. 631. 632. 633. 634. 635. 636. 637. 638. 639. 640. 641. 642. 643. 644. 645. 646. 647. 648. 649. 650. 651. 652. 653. 654. 655. 656. 657. 658. 659. 660. 661. 662. 663. 664. 665. 666. 667. 668. 669. 670. 671. 672. 673. 674. 675. 676. 677. 678. 679. 680. 681. 682. 683. 684. 685. 686. 687. 688. 689. 690. 691. 692. 693. 694. 695. 696. 697. 698. 699. 700. 701. 702. 703. 704. 705. 706. 707. 708. 709. 710. 711. 712. 713. 714. 715. 716. 717. 718. 719. 720. 721. 722. 723. 724. 725. 726. 727. 728. 729. 730. 731. 732. 733. 734. 735. 736. 737. 738. 739. 740. 741. 742. 743. 744. 745. 746. 747. 748. 749. 750. 751. 752. 753. 754. 755. 756. 757. 758. 759. 760. 761. 762. 763. 764. 765. 766. 767. 768. 769. 770. 771. 772. 773. 774. 775. 776. 777. 778. 779. 780. 781. 782. 783. 784. 785. 786. 787. 788. 789. 790. 791. 792. 793. 794. 795. 796. 797. 798. 799. 800. 801. 802. 803. 804. 805. 806. 807. 808. 809. 810. 811. 812. 813. 814. 815. 816. 817. 818. 819. 820. 821. 822. 823. 824. 825. 826. 827. 828. 829. 830. 831. 832. 833. 834. 835. 836. 837. 838. 839. 840. 841. 842. 843. 844. 845. 846. 847. 848. 849. 850. 851. 852. 853. 854. 855. 856. 857. 858. 859. 860. 861. 862. 863. 864. 865. 866. 867. 868. 869. 870. 871. 872. 873. 874. 875. 876. 877. 878. 879. 880. 881. 882. 883. 884. 885. 886. 887. 888. 889. 890. 891. 892. 893. 894. 895. 896. 897. 898. 899. 900. 901. 902. 903. 904. 905. 906. 907. 908. 909. 910. 911. 912. 913. 914. 915. 916. 917. 918. 919. 920. 921. 922. 923. 924. 925. 926. 927. 928. 929. 930. 931. 932. 933. 934. 935. 936. 937. 938. 939. 940. 941. 942. 943. 944. 945. 946. 947. 948. 949. 950. 951. 952. 953. 954. 955. 956. 957. 958. 959. 960. 961. 962. 963. 964. 965. 966. 967. 968. 969. 970. 971. 972. 973. 974. 975. 976. 977. 978. 979. 980. 981. 982. 983. 984. 985. 986. 987. 988. 989. 990. 991. 992. 993. 994. 995. 996. 997. 998. 999. 1000.

Post Paid

Doctor Al. Gray

Professor of Botany in Harvard Univ

Cambridge

W Boston

U. States America

Amud.
July, 19²

Temple Lodge
Hammer-smith - 8th February 1854

My dear Sir

I found yesterday at the Linnean Society your "Characters of new Genera" which you kindly sent for me, and there were also your kind presents of Part. 2. of your "Planta Wrightiana" & Char. of Gen. & Spec. of "Nyctagin²" which have been laying there a long while for me - for these I beg to return you my cordial thanks. I have hastily glanced over your "new Genera" which have much interested me. When at leisure I will refer to my notes respecting the Chilean *Myrtaceae* - I could never understand why our friend Hooker placed them (at least the species ~~with~~ with which I was acquainted in Chile), in *Eugenia*, and agree with you in separating them, but I do not perceive in what respects your Genus *Luma* differs from true *Myrtus*, unless it be in the thinner testa, but then, *L. (nuckshankiana)* and some others, have a crustaceous testa, as in *Myrtus*. I am not a little curious about your Genus *Rhytidandra*, and judging from the characters there given, am led to doubt the propriety of placing it in *Olacaceae* - at least I will take the liberty of suggesting these doubts to you - 1.^o because of the stamens being alternate with the petals, 2.^o on account of its epigynous corolla - 3.^o from its calyx being adnate to the ovarium and 4.^o for reason of its solitary suspended ovule, in a single-celled ovarium. You are probably aware that I have separated from the *Olacaceae*, all Benthams tribe of the *Leacineae*, and plants allied to them, placing them in a separate order, near *Celastraceae*; - they differ from *Olacaceae*, in having their stamens alternate with, not opposite to the petals, and in having 2 ovules collaterally suspended from near the summit of the cell of an ovarium, which is usually only 1-celled, but this arises wholly from abortion, for occasionally I have found the ovary 2 or 3-celled, and normally, it is probably 5-celled. You will find these ordinal distinctions briefly described in the late new Edition of the "vegetable kingdom" of our friend Lindley (p. 444). I have already published monographs of the 13 genera of the *Leacineae* and will furnish illustrated drawings of each in my "Contributions". You will also see there (p. 444) my views of the structure of the true *Olacaceae* - monographs of all the genera there given are completed, but not yet published: here the ovarium is also superior in respect to the calyx, but in *Schöppfia*, the ovarium is half invested by an adnate fleshy disk, on the margin of which the corolla is supported, and this disk or cup is quite free from the calyx, so that the ovarium is quite superior with respect to the calyx,

and only half superior in regard to the corolla: here also as in the rest of the family, the ovarium is unilocular at the summit, and half 3-celled at the base, and the ovules are suspended from a free axile placenta, - not suspended from the summit of the cell: here likewise (as sometimes occurs in *Laciniaceae*) there is a remarkable ~~and~~ large epigynous disk, ^{or gland,} besides the ~~conspicuous~~ fleshy hypogynous disk! The same fact I noted also in *Hyoscyameae* and in other cases - a fact of not infrequent occurrence, though contrary to the received theory. - The circumstance of the suspension of a solitary ovule from the summit of a single-celled ovarium, coupled with many other obvious features, appears to me to bring your genus much nearer to the *Loranthaceae*, which family I have also briefly characterized *beg. Kungd. p. 791*. Your Characters of *Rhytidandra* quite agree with those of *Struthanthus*, except in the circumstance of the position of the stamens, which are opposite in the latter genus, not alternate with the petals: the peculiar structure of the anthers, which you mention, agrees also with that genus, and is somewhat analogous to that of the *Biscaceae*. I will look with attention over the *Planta Wrightiana* & the *Nyctaginaceae*, for there is much analogy between these plants and those of the portions of So. America I have visited, so that I cannot fail to take more than an ordinary degree of interest in them.

During the last 12 months, I have been able to attend very little to Botanical pursuits, having been much engrossed by matters of business, from which I hope to be quite free again in 2 or 3 months. I have therefore been rarely to the Linnean, or Museum, and still more seldom to Kew, which I have visited only twice in the last 6 months. All I have been able to do, is to complete my monograph of the genus *Lycium*, of which I propose to figure all the species (upwards of 60), and with this close the 2nd vol of my "Illustrations", which I will forward to you when completed. After that I will get on with the "Contributions", the plates of which are entirely in arrears, and which I shall have the pleasure of sending you when finished - I must also use some efforts to ~~publish~~ the *Menispermaceae*. This brings to mind a request, of which I take the liberty of reminding you. You will remember sending me a flower of a species of *Clypea* from Tahiti, to examine, which I felt anxious to know if it differed from Forster's plant. Should you have an opportunity of referring to it, I will beg the favour that you will trace in pencil on your letter, a rough outline of the leaf & general habit of the plant, with such short observation on its pubescence &c as may be necessary to identify it.

I am now at work, whenever I can spare a few hours, upon the *Clusiaceae*, confining myself chiefly to the Guttiferous plants of the New World, and hope to define better the limits of the genera of this family, toward which end I made several observations, and collected many facts, during my residence in Brazil. I shall be able to show demonstratively, that Botanists all along, have been quite mistaken in regard to the structure of its Embryo: - that the part which has been considered as radicle, consists of 2 minute Cotyledons, - the true radicle being the large mass of the nucleus, which has been held to be the confluent cotyledons by most authors, and albumen by Goertner and Martius. This structure brings the *Rhizophoraceae*, as Lindley shrewdly concluded, in close contact with the that family, and draws still closer its connexion with *Hypericaceae* and *Ellaragraceae*. As you do not describe any member of the Guttiferous family in your "Genera of the United States," I conclude you consider that no plant of the order is found within the limits of your extensive regions - nor am I aware that any one is to be met with, north of the Isthmus of Darien: - still however, *Clusia rosea* is figured in Catesby Carol., and quoted by DeCandolle as being a native of Carolina, as well as St. Domingo. I wish much to know the actual structure of this plant of which I find no where, any accurate description: I suspect that all our Brazilian *Clusia* will be found generically distinct, not only from this, but from Aublet's Kunth's & Martius's plants bearing the same name, which again appear to me essentially different from the original and typical species of Catesby: - of this latter plant, no specimen exists in the collections of the British Museum, but I hope to be more successful in my search for it at Kew, when I can find time to get there.

I shall be glad to hear that Mr. Gray and you have enjoyed good health - Mr. Miers and my daughter desire to unite with me in very cordial regards to her and to yourself & in the hope of having the pleasure of meeting together again at no great distance of time.

Believe me to be my dear Sir always very truly, Yours

John Miers

Doct. Asa Gray

Temple Lodge - Hammersmith
Wth London 28th October 1854

My dear Doctor Gray

Your two "greatly" esteemed favours of the 18th & 21st July reached me 3 days prior to my departure from London, when ~~having~~ much business and pressing correspondence to finish for the Brazilian mail I was unable to write to you in reply. I now however avail myself of the first leisure moments since my return to thank you for your kind letters and their enclosures, of a seed of *Calycocarpum dizonii*, a specimen of *Buckleya*, your memoir on that genus, and a fragment of "*Stephania* Forster," all which have greatly interested me. I made an analysis 4 years ago of a seed said to be of *Calycocarpum*, which I owed to the kindness of M. de Boissier: your specimen though not ripe enough to exhibit the form of the embryo, has been of much value by showing that I have been correct in the results formerly obtained. This seed differs essentially from that of *Odontocarya*, which wants the singular cristate margins, and has only 3 prominent teeth at each extremity. I thank you for the leaves & flowers of your Polynesian Menispermaceous plant, which on account of its quaternary arrangement I fancied might be referrible to the genus *Clypea* of Blume, but this cannot be - In *Clypea* I have found the flowers crowded on a pettate involucre - the petals singularly bullated and winged, and the anthers only 2-celled. I have observed the flowers of *Stephania* constantly 3-merous, but if as you say, the flowers in your plant be both 3 and 4-merous, it may throw some doubt upon the correct reference of some species of *Cyclea*, ~~of~~ which the male flowers differ from those of *Stephania* only by the number of the parts. In the seed, their great difference is at once detected; - the nut of *Stephania* is flatter, always with a distinct perforation in the middle of the Condyle, or central hollow flat disk round which the cell is coiled: in *Cyclea*, the Condyle is very thick, convex, without aperture, and forms a close chamber. Your plant from its habit, and its usually 4-merous flowers, I am therefore disposed to refer rather to *Cyclea*.

The specimen of *Buckleya* is extremely interesting, and its value is much increased by your observations on that genus. The presence of a double row of floral envelopes, with the outer series so many times longer than the inner whorl, is very curious, and quite at variance with the usual arrangement found in the Santalaceae, to which order I fully agree with you, in referring the genus: we have however in *Mida*, *Choretum*, and *Nausodea*, something

approaching to the nature of a double envelope, for the segments of the border are articulated on the margin of an obsolete 5-toothed adnate calyx, the lobes alternating with the teeth. I can perceive nothing in the ostivation of *Buckleya*, at variance with that ~~constant~~ of *Santalaceae*; and it appears to me completely that of *Choretrum*, and some other genera, not exactly valvate, but somewhat involute in addition - the dried specimen appears to be so, and not imbricate . But in general cases, I attach little value in itself to the character of ostivation, however important it may often prove as an accessory feature. This partially involute mode of ostivation (which exists also in *Scacinaceae*) is general through ~~both~~ *Olacaceae*, and often *Santalaceae*, and is frequently carried to such an extent near the summit of the petals, that their several apices are often ~~combined~~ inflected in the shape of a pendent process, extending half way down the petal, and these appendices meeting in the axis of the flower, cohere valvately together in a central mass, hanging in the middle of the stamens before bursting. . You allude to the circumstance of my having excluded *Bursinopetalum* from *Scacinaceae*, on account of its ostivation: I believe I was in error in doing so; it was not however because of its ostivation, which is exactly that of *Scacinaceae*, but of the striking resemblance of its fruit to that of *Villaresia*, a genus referred by botanists to *Aquifoliaceae*. I mean to examine the matter more cautiously, and may probably be disposed to refer *Bursinopetalum*, *Villaresia*, and *Oxtoxon*, to *Scacinaceae*, although the latter genus has a decidedly imbricated ostivation. This family, it is now evident, has no relation whatever with either *Olacaceae* or *Santalaceae*, and is distinguished by a normally plurilocular ovary, with 2 ovules suspended from the axis, near the summit of each cell, although generally only one cell becomes developed, and only a single seed matured. You kindly promise me a sketch of your genus *Rhytidandra*, and a tracing of a new species of "*Pleuronopetalum*" which I shall be very glad to see. The few characters hitherto published of Blume's *Pleuronopetalum*, differ in no respect from those of Dr. White's genus, so that I have always considered the former, to be merely a synonym of *Bursinopetalum*: if this be the case, *Pleuronopetalum* must remain valid for Doct. Hooker's genus, long since published, and clearly belonging to, *Portulacaceae*, but which was I think, without reason, referred to *Amaranthaceae* by Endlicher and Moquin, and by the latter, injudiciously changed to the name of *Allochlamys* (DC. Prodr. xiii p. 463). Referring to what you say of your species of *Pleuronopetalum*, the structure of the ovary will at once decide, whether it belong to *Scacinaceae*. You will often find much external ^{and general} resemblance in the flowers of this family, with

those of some genera of *Loranthaceae*, so much so, as to have deceived an experienced botanist as Doct. Planchon.

In my last I mentioned that my attention had been directed to a more extensive examination of the *Euttiferae*, upon finding that my observations made in Brazil, upon the plants of that family, were not in accordance with the facts upon record. At the last meeting of the Linnean Society in June, a portion of my paper was read, on the structure of the embryo in the *Clusiaceae*, and I will here allude to a topic, which I there discussed, that has naturally resulted from my investigations, where I have taken the liberty of differing in opinion on a point of structure which you seem to have adopted on the authority of Endlicher. In your most excellent work (*Gen. Un. St.*) you have described the exterior tunic of the seed of *Magnolia*, as its testa, and the thick crustaceous covering as the tegmen, justifying this conclusion on the appearance of spiral vessels in the placentiferous attachment of the anatropal ovules. I have combated this idea upon what appears to me solid grounds, and I endeavour to prove the crimson fleshy envelope to be a true arillus, because there exists a distinct free raphe within it, proceeding from the basal point of attachment, and lying between the arillus, and one side of the crustaceous coating, and disappearing at a point in the summit of this hard coating, which I consider to be the testa, through an opening (which I have called the Diapyle in contradistinction to the micropyle, ^{visibly} at the opposite extremity) which it penetrates, and where it terminates in the darkened Chalaza of a more internal integument (the true tegmen) a covering which you and Endlicher seem to have overlooked: this diapyle, of course points to the cotyledonary extremity of the embryo. Such was my analysis many years ago of *Talauma*, a genus little differing from *Magnolia*. It was of some importance to me to settle this point, in reference to my investigation of the *Clusiaceae*, because there exists in the seeds of the tribe *Clusiaceae*, an exactly similar coating which I hold to be the arillus, investing a crustaceous coating which I conclude to be the testa, for whatever be its nature in *Magnoliaceae*, must be the same in *Clusiaceae*; and I infer ^{the former} it must be arillus because it is always exterior to the free raphe. It is clear that if the scarlet arillus were a development of the primine, and the thick crustaceous coating were produced from the secundine, the raphe could not exist in a free state between them; it must necessarily be exterior to the production of primine, and whatever coating exists in the seed, exterior to the raphe, must be of subsequent extraneous growth, and therefore arillus. This view of the matter is further proved, by the condition of a corresponding tunic in the tribe *Toromiteae*, where it is also crimson and fleshy, but not entire, as in the *Clusiaceae*, the margins being free, and overlapping each other.

along the dorsal line of the seed, which it encloses entirely: in the *Garcinea*, the same development assumes the form of pulp. Hence it appears to me clearly demonstrated, that in the *Clusiaceae*, as in the *Magnoliaceae*, the external crimson soft tunic is arilless, the hard crustaceous coating the true testa, with a well marked perforated diapyle near its summit, through which the extremity of the more external raphe penetrates, to terminate in the Chalazae of the inner membranaceous tegmen. In all this, there is nothing out of the ordinary rule, such being the usual structure found in *dicotyledonous* seeds. This matter became of additional interest to me, because Doct. Thomson informed me, that he and Doct. Hooker, in their forthcoming *Flora Indica*, had adopted your view of the nature of the tunics in the seed of the *Magnoliaceae*.

On my return home I had the gratification of finding a letter from your friend and able collaborator Doct. Torrey, which I have much pleasure in answering. This brings to my mind another enquiry. There is often, as you will know, a great analogy between the plants of Chile, and California or other parts of Northwestern America, and as you have studied the botany of these regions, particularly the *Compositae*, you will probably know whether any of the *Calyceraceae* have been found there. I make the enquiry, because I have collected the materials for a more complete monograph of the family, which I propose to illustrate one of these days.

We have had a most delightful trip to Switzerland again this year, and were greatly charmed with parts of the Valais, we have not before visited, especially the valleys of Zermatt and Saas. There is here a perfect circle of extensive glaciers, and snow-capped peaks of extreme elevation, all close around, - including Mischabel, Monte Rosa - M. Cervin - Weisshorn and many others, the Panorama being rendered complete, by the more distant Jungfrau and higher Bernese alps. In my estimation, it is the most magnificent scene I have ever beheld, far more striking than the justly celebrated views about Chamounix. Mr. Miers and my daughter unite with me in very kind regards to Mr. Gray and Joinsel, and believe me always, very sincerely yours

John Miers

Am. 5

Hammersmith 5th June 1855.

My dear Doctor Gray

I have long delayed writing to acknowledge the receipt of your kind favour of about Christmas last which I had carefully put aside for reply, but which has somehow got mislaid: The delay arose from the desire to acknowledge the receipt of the works which you then announced you had kindly sent to me through the care of Sir W^m Hooker. I was at Kew 3 or 4 times in Jan^y & Feb^y enquiring for them, but they had then been mislaid; since that time I have been confined within doors, first by indisposition and secondly by a sprained ankle which accident kept me a prisoner nearly 6 weeks so that I was not able to renew my visit there till very lately, when I was glad to obtain your "note on *Vavda*" and "*N. nov. Thurberiana*" which had at last turned up, and ~~had~~ been put aside for me and for which I return you my very best thanks, as well as for the seed of *Calyocarpum* which you kindly sent me some time ago. I have also to thank you for your last kind note of 9 April last announcing your having forwarded through Mess^{rs} Trübner & Co the memoirs Doct^r Torrey had so obligingly sent to you for me which have come safely to hand, and for which I have written to offer him my very best acknowledgements. Your "note on *Vavda*" especially interested me as you have there commented on some observations I made on *Styracex*, to which I will here again in the same cordial manner, that I much regret you have not been able to coincide with what I have so distinctly met with in the structure of the ovary of *Styrax*. My observations on that family were only general, and in relation to its affinities with *Olacaceae*, it being my intention at some future time to examine more in detail the plants comprised in it: all I can say is that I certainly met with the structure depicted in the "*Beg Kingd.*" and that you will find it confirmed in other quarters as I will presently show. I think you have not perused all I have said on the subject, or you would have looked at the matter differently. The circumstance of the normally incomplete union of the carpels in that family, which you appear strongly to doubt, is I think rendered even more manifest by your own argument; you have confirmed my observation of the upper row of ovules being erect, the middle horizontal, & the lowermost pendulous in the several species you

examined, but you add this is not true of the whole genus, as Zuccarini found them all erect in one instance, as you seem to think may also be the case in some American species, a consideration that appears to have much influenced your opinion. But it appears to me that mere direction of the ovules in no way affects the question, and that this is of little consequence, even in a generic, much less in an ordinal point of view, for if in any species the placentary column, which unites the bases of the incomplete dissepiments, be shorter than usual, the mere pressure of the ovules on each other would cause them to assume an erect position, in which case the ovary would still be 3-locular at base and 1-locular above. This (I am surprised you say) you do not find to be the case, which is the more unaccountable, because you admit at the same time, the accuracy of Defandolle's definition of "parietibus incompletis ab axe centrali demum distantibus": now these semi-partitions are thick and solid, and would not become "demum distantibus", if there were any real union of their edges with the axis:—indeed the discontinuation of the placentary column along that line, which you admit, tends to confirm the view I have taken, that no absolute union has there, at any time, taken place.

In every species I have yet examined (which I admit are comparatively few), I find no union of the incomplete dissepiments above the line of the ovuliferous column. This structure so nearly approaches Humiriaceae, and differs normally, and so widely, from that of Symplocaceae, that taking into consideration the constant and striking difference of the structure of their stamens, the great dissimilarity of their embryos and seed, and other characters, we have the strongest evidence to warrant the separation of Styraceae from Symplocaceae, and no solid foundation whatever for their approximation, much less for their union. Upon nearly similar grounds, they differ as widely from each other, as the Icacinaceae from Olacaceae, if the facts I have stated, be founded on truth.

You do not admit the existence of the epigynous glandular swelling in the ovary of *Styrax*, although you confess to a thickening of the epidermis there. This fleshy appendage, which I have observed in *Styrax*, is very different in its nature from the epigynous disk found upon most inferior ovaries: it is without doubt a distinct fleshy deposit, not a mere thickening of the epidermis, as you suggest, and is a circumstance of no uncommon occurrence upon completely superior ovaria, as I have frequently pointed out; it often exists concurrently with a thick hypogynous disk, and I have called it on all occasions, if I mistake not, an epigynous gland, to distinguish it from the true epigynous disk, whose true

nature was I think pointed out by Mr Brown. It is largely developed in Icacinaceae Olacaceae &c where in *Schöppia* it forms a cushion as large as the ovary itself. I have also described its existence in *Flyoseyamus*, which to this cause owes the circumscissile dehiscence of its capsule. The epigynous disk is largely developed in the Brazilian *Symploca* (*Stemmatosiphon*) and is seen in the living state much larger than is shown in Mr Bentham's analysis from the dried specimen, it is fully as high, and of much larger diameter than the ~~ovary~~ inferior ovary with about 20 deep grooves and is of a dark orange colour. Among Symplocaceae I have also a genus yet unfigured where the parts are 5-merous, the stamens 15, uniserial as in *Barberina*, but the ovary is 2-locular, with a single suspended ovule in each cell, one of which always becomes abortive, the fruit is a dry indehiscent chartaceous unilocular putamen with a single seed: in another genus the corolla is 3-fid, the stamens uniserial, are 6 or 9, the ovary is 3-celled with a single suspended ovule in each cell.

You object to my statement that the ovary is inferior in Symplocaceae, and superior in Styraceae, and urge that this is not true, at least in *Pamphilia*, where you say, the ovary is not superior; but on this point, it appears to me, you are evidently under misconception, as you will feel convinced if you refer to Delessert's Icon. Select. 5. tab. 42, where you will see in the analysis of *Pamphilia styracifolia*, the ovary depicted as being wholly superior, the cell 3-locular at base, unilocular above, and the ovules all erect, as I have above-stated would happen where, as in this case, the placentiferous column is shorter than usual. You will also observe a repetition of these same facts in the following plate of *Foveolaria*, all confirmatory of the views I have stated in regard to this family. You remark moreover, that a superior ovary is denied in the case of *Pterostyrax* and *Halasia*: in the former, it is however described as being "semi-superior", which is about the same thing; while the latter genus was considered so abnormal by Endlicher, both from Styraceae and Symplocaceae, that he formed it into a distinct suborder (*Halasia*). I have never seen any specimen of either of these genera, so that I have no positive evidence on the subject. — In stating that the stamens are few

and uniserial in *Styraceae*, and pluriserial in *Symplocaceae*, I asserted only that which is generally the case, for I was aware of occasional exceptions to this rule in the latter family, as in the instances I have above described within my own experience, and even in *Barberina*, where though apparently in a single row, the 15 or 20 stamens were in all probability ^{normally} 3 or 4-seried, but owing to the narrowness of the filaments, obliged by pressure to assume an uniserial position. The fact, one way or the other, is of little importance to the main question, but the circumstance of the adnate character of the anthers in *Styraceae*, and their being oscillatory in *Symplocaceae*, is one of far more consequence, a difference which I believe to be ~~universal~~ constant, for although as you remark, the anthers in *Pterostyrax* may be shorter than in *Styrax*, they are still described as being adnate. You will therefore I am sure excuse me when I say, that I do not perceive in your several observations one tenable argument, or fact, that tends to invalidate the evidence I have shown, or that can annul the inferences which naturally follow as a consequence.

You express surprise that I should have ignored any affinity between *Styraceae* and *Meliaceae*, but if this was not shown, it was manifestly inferred, by my conviction stated in positive terms, of its real affinity towards *Fluminiaceae*, as indeed had been long before suggested, though upon different grounds, by Martius and Benthams, and thus of course to the *Meliaceae*, between which families there exists a most obvious relation. I was well aware that DeCandolle in his *Prodromus*, had placed *Toveolaria* in the latter family, as had been proposed many years previously by Jussieu where he also associated *Styrax*. It escaped my memory at the moment, but it soon after occurred to me, that this connexion was rendered still closer by the observations of Jussieu, showing that in the *Meliaceae*, the cells of the ovary are in like manner unilocular at the summit, where the dissepiments are incomplete, or as he calls them pervious. You must not imagine that my suggestion for the removal of the *Styraceae*, from the position hitherto assigned to this family, to the neighbourhood of the *Fluminiaceae*, was either arbitrary or speculative, and I will endeavour, in a few words as possible, to show you the reasoning that led me to adopt it. — In seeking for the soundest views

of systematic arrangement, it always appeared to me that the Characters drawn from the organs of reproduction (as being most constant) should always claim the preference, and the most important of them is certainly the ovarium, the organization of which is generally believed to be either simple or formed of various combinations of different carpels, each carpel resulting from a peculiar modification of the leaf, some portions of which, generally the interfolded margins, are supposed to become ovuliferous. Upon this hypothesis we have readily explained the source of the numerous kinds of parietal axile or projecting placentations but ^{it} does not account for the free central placentation.

This I have imagined to proceed from the point of junction of the petioles of the combined carpellary leaves, the margins of which remain sterile; while in the case of the partially many-celled ovary unilocular only at the summit, I have assumed it is the basal portion of the carpellary leaves alone that are fertile, while the upper portions remain sterile and disunited. Upon this hypothesis the proper place in the system of the group I have called Cionospermeae and which is now become extensive, will naturally fall into order following those families exhibiting a gynophorous and bicarpellary ovarium ~~near the head of which~~ and preceding those possessed of a more perfectly many-celled ovary near the head of which the Olacaceae and Humiriaceae have hitherto stood and close to where the Meliaceae have been located. a somewhat analogous structure occurs also in Bombaceae as was long ago indicated by Vahl to exist in *Evodendron* and *Chorisia* (I mean an ovary always more or less unilocular at summit and 3 to 5-celled at base) that of *Pachira* he also describes as being "serius quasi 1-loculare", and it is probably so also in *Bombax*. While in Rio I had an opportunity of studying the unilocular structure of the ovary and the remarkable development of the funicles and seeds in *Adansonia* - it is very singular and is most imperfectly known and described but I may some day publish it from the complete drawings I made many years ago. All that I have hitherto stated in regard to *Styracae*, *Symplocarceae*, *Ebenaceae* has been merely indicative my object being to show they have been sadly misplaced in the *Prodromus* of DeCandolle. a more critical and patient investigation of these families is necessary but I think I have shown enough to point out the direction into which they will severally fall.

Your observations on *Rhytidandra* are very interesting and excellent, and your reference of the genus to the *Alangiaceae* doubtlessly approaches the truth. From the short character you had given of the genus, the scandent habit seemed to point more directly to *Loranthus* than to *Alangium* or *Markea*, but in commenting on my suggestion of its probable relation to the former genus you do not appear to have borne in mind what I had previously said of that genus "Alangium, as suggested by Dr. Wright seems to approach nearest (to *Loranthus*) in its structure, with its adnate calyx linear petals with valvate ostivation, adnate anthers, large epigynous pulvinate disk, simple style, 1-celled ovary with a single ovule suspended from its summit its monospermous drupe with the seed suspended having an orthopetal embryo enclosed in albumen, a superior radicle and large cotyledons: to these may be added its alternate frequently punctate leaves and axillary flowers" (Bey. Kunt. 179. b.) In the diagnosis you give of *Rhytidandra*, one of its most singular features is the structure of its anthers "anthera lineares introvsum adnatae dithecae 4-locellatae locellis annulato rugosis vel cameratis" which you explain in saying "its moniliform and chambered anther-cells": this peculiarity I do not remember to exist in any other instance than in *Agiceras*. In *Loranthus* however the structure of the anther is worthy of notice - it is always 4-locellate and often dithecal from the combination of the cells in pairs, but at other times the 4 linear cells are quite distinct and parallel, being separated by equal spaces, thus forming a half cylinder round a fleshy connective. The cells themselves are of peculiar structure texture, the walls are thick & crystalline consisting of numerous square cellules transversely & longitudinally parallel. The same structure occurs also in *Olacaceae* & *Santalaceae* and in the latter family these cellules in some of the sterile anthers are so large that they might be mistaken for the pollen cells like those attributed to *Biscum album*.

June 16th - I will add one remark here, for fear what I have before said, be misunderstood. In all cases of botanical analysis I always endeavour to avoid being guided by mere external appearances and seek, wherever practicable, to trace the development of structure through its several stages: this rule has proved useful in the present case, for we have many instances where the ovary is plurilocular at base and unilocular at summit, for instance in *Bombacaceae* but there exists a wide distinction in the normal structure of the ovary in this family and that of the others to which I have above referred, such as *Styracaceae*. I have already trespassed too much on your patience to enter now on this discussion, but shall probably explain my views more fully, when I come to treat of these families, as I hope to do at some future time. I was led to make

this comparison by the appearances presented by *Adansonia*, of which I had the good fortune to obtain both ~~a~~ flower and ~~a~~ fruit in Rio de Janeiro and the structure of which I find to be most imperfectly described. There and in other genera of this family which I have examined, circumstances occur, not yet noticed, which offer many important considerations, for they serve to harmonize and reduce to one common origin, developments which in progress of their growth, appear to contradict each other. - In my memoir on the structure of the seed of *Eutliferia*, I made several remarks on the nature of the arillus, and combated your opinion that the fleshy external coating of the seed in *Magnoliaceae* is not arillus, but testa; I then alluded to the fact, that the arillus is not always recognizable by its substance or texture, being even sometimes hard polished and crustaceous, as in *Drymis*, and in support of this opinion that what has here been considered as the testa is an arillus, I have lately been working out my old analysis of this Genus. - I have also been examining *Canella alba*, which I find will occupy quite a different position in the system, to that hitherto assigned to it. - I have also been looking out during my confinement at home, my various analyses of Brazilian *Apocynaceae* plants, some genera of which are new, and others imperfectly described, and my late visit to Kew has enabled me to resume the investigation of *Clusiaceae*. You will therefore see I have not been idle. My several attempts to find an artist competent to copy my drawings on stone, have been unsuccessful, and I envy your facilities in the assistance you have from Sprague and Prestele the plates in the *Planta Fremontiana* are beautifully executed - I hope yet to succeed in finding some one to assist me in completing the plates of the 2nd Vol. of my *Illustration* which continue sadly in arrears. I must apologize to you for the length of these observations, by which I have been carried away, for when I began, I did not expect they would extend to near this length. - You will see by the dates, that I was interrupted, my time in the interval having been wholly devoted to the affairs of my sons in Brazil, which pressed for my immediate attention. Mr. Miers and my daughter unite with me in very kind and cordial regards to Mr. Gray and yourself: the time is drawing near for our annual visit to Switzerland whence on our return we propose to make a short stay in Paris to join in the general attraction of the Great Exposition. - we should have rejoiced if this temptation had enticed you to pay another visit to Europe - we may yet perhaps hope for the pleasure of seeing you. Believe me to remain

very truly Yours
John Miers

Hammar-smith 20 December 1855

My dear Doctor Gray

I take the first spare moment after my return from Birmingham to acknowledge the receipt of your greatly esteemed favour of the 8th ult^o. by which I was surprized to hear you had been so lately in London and Paris: we must have been in the latter City about the same time where it would have given me much pleasure to have met you. I returned home in the beginning of October after a delightful tour in Switzerland & Piedmont stopping in Paris both ways, and had hardly been a week at home, when I was taken ill and confined nearly 2 months so that I have not yet had the satisfaction of seeing any of our friends at Kew, or I should have learned from them that we had been in such close proximity. I am obliged for your remarks about the ovule of Magnolia, and although we are still at issue about the nature of the fleshy coat of its seed, it is gratifying to me to have to contend with so able and generous an opponent: I feel confident however that as the discovery of truth is our sole object, we shall soon be of one accord, & if I am in the wrong I will hasten to acknowledge my error. I had already combated your very able and ingenious arguments published in Hooker's Journal for August, in a paper now in the hands of Mr Bennett, which is not yet read before the Linn. Soc^y, but which I will not withdraw, as there is nothing in your letter that induces me to alter any opinion there expressed. I have there taken a wider view of the whole matter, and have entered into the question of the raphe under its many varying circumstances of development. I was led into this by the puzzling question that occurred to me in the investigation of the Clusiaceae - why should the raphe be quite free from the testa as a simple cord in the Clusiae, while in the Tovomitae and Garcinieae it appears as branching nervures spread all over and imbedded in the substance of the Testa? There appears here an incongruity in the position of the nourishing vessels of the future raphe in regard to the primine which at first sight is inexplicable: the same difference

has always puzzled me from the earliest period of my acquaintance with structural botany, and the explanation of which I have sought in vain in botanical works, or in the various memoirs that treat on the development of the ovule and seed. I think I have successfully reduced these apparent incongruities to one simple law of development, but I cannot give you in this letter all the facts adduced on this subject - let me add that our discussion on the nature of the arilliform coating of magnolia seeds, has led to the solution of this difficulty. I make a distinction between the different kinds of extraneous tunics in various seeds: - those which as in the *Trochites* &c are produced immediately from the placenta or funicle, I consider to be a true aril, in which sense I would confine that term, - but I would employ the name of arilline to such coatings as exist in the *Clusiaceae*, *Magnoliaceae* &c, which I hold to be a development of the placental sheath containing the nourishing vessels of the ovule which you figure in *Magnolia* fig. 5-6 & 7. Although you failed to notice the extension of this sheath (in looking for the growth of the outer coating from the expansion of the placenta) we know that such extension must have taken place as it has been seen to be developed in other cases. I believe it to be identical with the arillode of Planchon who watched its growth in *Euonymus*, where the scarlet coating is quite analogous to that of *Magnolia* and where the raphe is found exactly under the same circumstances - he imagined it must ~~proceed~~ have originated in an extension of the foramen of the ovule, though he states that the latter was hidden at the time by the placental sheath but if he had reflected a little more, he would have drawn the more natural inference that the fleshy tunic which he saw growing in different stages of its growth, proceeded from the extension of the placental sheath. ^{in regard to the aril} St. Hilaire (whose views are supported with some modifications by his pupil Planchon) describes and figures (*Flor. Braz. Mer.*) the incomplete formation of the arilline in *Turneraceae*, proceeding from the upper portion of the raphe or placental sheath, its lower undeveloped extremity being still visible as a cord attached to the testa, and both Planchon and Gasparini at different times actually witnessed the progressive and complete formation of this coating in the ovules of *Opuntia*, which they describe with much minuteness under remarkable similarity of circumstances - they both describe the extension of the placental sheath in the form of an extremely delicate membrane whose margins gradually unite and which progressively thickens so as to ~~form~~ constitute

an entire tunic. — In cases however where the arilline is formed as a mere pellicular extension of the placental sheath, and where the quantity of nourishing vessels abound in excess, these branch out and are extended with the growth of the arilline over the surface of the primine, now enclosed within the extended sheath: where the latter has no secretion of fleshy tissue to thicken its substance, but where mucous deposits intervene between the arilline and primine, the result is, such as we see in *Trochites* for instance, an apparently simple, but really compound tunic in the seed, with a branching raphe interposed between the adherent membranous arilline and true testa. A very instructive proof of this exists in the seeds of *Tesseraudia* and *Olea*, where the kernel is enveloped in a somewhat thin chartaceous testa which appears to be a simple tunic, but if this be well macerated, it is found to be double, the 2 coatings being easily separated, having a dense net-work of delicate white threads intervening between them, which may be drawn out in the form of beautiful spiral vessels, thus exhibiting a peculiar modification of the raphe. You will see we are somewhat approximating in our ideas as to a difference between the aril and arilline.

You consider the latter (not as formerly as the developed primine, but) merely as an epidermis of the nut which latter you now admit to be the testa: while I contend this cannot be so, because of the position of the raphe. At the same time I hold the scarlet covering to be different from a true aril, though both are of placental origin, the one being a development from the placental cord, the other ~~from~~ the placental sheath: - both are observed facts. Under this view the many phenomena attendant on the growth of the extraneous coatings of seeds, and of the varied form and position of the raphe may be satisfactorily explained by one simple rule without violating any known law of development. When you take all these facts into consideration, I think you will come to the same conclusion. The fact you noticed of the deposition of osseous cells in the substance of the primine, simultaneously with the growth of the arilline, is very interesting, but it tends rather to confirm ~~than~~ rather than to rebut my arguments.

I am greatly obliged for the flowers of *Styrax* which you enclosed for me. I have some time ago prepared a memoir to show the very decided structural

differences that mark the *Styracaceae* which will be followed by another on the distinctive characters of the *Symplocaceae*. In the former I have noticed the observations made in your "notes on *Vauvea*". I will not publish this until I know something of *Kalesia* & *Pterostyrax* neither of which I have yet seen; and shall therefore gladly avail myself of your kind offer to send me seeds of *Kalesia* and better still, if you can spare me a fragment with flowers on it. I now enclose you a tabular view of the leading characters of these 2 ~~groups~~ families, placed side by side by way of contrast, and this is accompanied by sectional analyses that illustrate their extreme differences. There may exist a shade of variety in some of these features at times, as you have shown, but the main facts are true, I believe, in all cases - the only doubtful point remaining in *Kalesia*, but that incongruity may perhaps be solved by an examination of its structure. Don you know made a distinct family of it apart from *Styracaceae* but even if retained in the latter it would not be fatal to my views, for in the very natural order of *Melastomaceae*, as well as in others, we find both superior and inferior ovaries. I enclose you a flower and section of *Styrax laevis* where you will see the vase shaped superior ovary found in all the Brazilian species you will there perceive the thickened apex which I have figured, protruding in an umbracular form over the lower tubular portion - the very fleshy apex is however still more strongly marked in one of Gardner's species, (his n^o 4892 from Minas Gerais) which I also enclose. I had no idea of intimating that in *Styrax* the ovary is surmounted by a fleshy disk; all I wished to show was, that the summit of its shell is thicker in substance than the lower portion and glandlike in appearance, as in *Hyoscyamus* and in many *Oleaceae*, but this wants the difference of colour and texture and is of a nature wholly distinct from the conspicuous fleshy lobed cushion that crowns the inferior ovary of *Symplocos* (*Stemmatostichon*) - that is a true epigynous disk which is never found in *Styrax*. In the flowers you sent me the ovaries were completely crushed, but enough remained of the fragments to show the existence of the same form as in *S. laevis*, ^{that} the ovaries

are crowded about the central placenta, and that the ovary is distinctly
1-locular in the summit. I did not mean to say that the direction
of the ovules influenced your opinion regarding the affinities of the
Styracaea, but as you remarked that my statement of the ovules
being sometimes pendent or horizontal as well as erect, was not
borne out by your own observations, and that Zuccarini had
figured them all erect, I wished to ~~show~~ ^{show} this might (as indeed does)
sometimes happen, without affecting the nature of the structure,
for if the placenta be short, all the ovules must necessarily be erect,
if longer, they partake of the 3 several directions, as shown by me,
merely from the effect of mutual pressure. — The fact you
noticed of the deposition of osseous cells in the substance of the testa
of Magnolia, simultaneously with the growth of the fleshy envelope,
is very interesting, but it tends rather to confirm, than to rebut
my argument.

28th Decemr. Since writing the above, having many other matters to attend to,
I have been pondering upon Kalesia, and have looked to the descriptions of
Gaertner & others, relative to the structure of its fruit, and if I understand this
rightly, the 4 cells are complete, both the dissepiments and the axile column
become greatly thickened and osseous, the latter hollow in the centre, showing
a disposition to separate into 4 carpels, probably from the introflexion of its
dissepiments: indeed Endlicher asserts it to be "4-pyrena, pyrenis inter se distinctis",
and Gaertner so figures them: the seeds are erect or pendent (according as an erect
or suspended ovule has become perfected) and they are attached to the axile placenta.
Assuredly there is here no analogy whatever, with the fruit of Styrax, indeed it
shows that of Kalesia derives its origin from a very different kind of ovary. If
I might be allowed to guess at the affinity of a structure which I have never seen,
and which I know only from imperfect description, I would suggest a greater
analogy with Deutzia, or other genera of the Philadelphiceae: in the shape of its
multilocular putamen, terminated by the hardened persistent style, it agrees with
Decumaria & Deutzia, with which it also accords in the form of its albuminous embryo.

Symplocaceae continued

Ovary wholly inferior - crowned by a large pulvinate grooved coloured fleshy disk: rarely 3- or 2-celled, cells complete, their axis being continuous with the style. Cells always inferior, but the ovary appears often semi-superior by the protrusion of its large epigynous disk.

Ovules 2 to 4 in each cell superimposed - suspended from the internal angle of each cell near the summit of the axis.

Style simple elongated

Stigma capitate with as many lobes as cells in the ovary.

Drupe inferior, generally somewhat fleshy the sarcocarp being formed of the persistent calyx, and crowned by its small persistent limb, having an umbilical depression in the centre remaining from the epigynous disk. Nut generally hard and long, mostly 5- rarely 3-celled. Sometimes coriaceous and 1-celled by abortion, indehiscent.

Seed always suspended - 1-often 4 in each cell, long, cylindrical pointed at each end.

Testa and Integument in one simple and membranaceous tunic

Albumen copious fleshy

Embryo slender terete, axile

Radicle nearly the length of the whole superior pointing to the hilum

Cotyledons very minute, equal in breadth to the radicle

Styracaceae continued

Ovary wholly superior, turbinate, apex conical or flattened and projecting in an umbracular form - often thickened and gland-like in this part - briefly 3-locular at base, above this the dissepiments are incomplete and continued to near the summit, like narrow projections, leaving the middle and summit of the ovary 1-locular. The placenta springing from the base of the ovary rises above the line of the complete portion of the partitions in the form of a free central column to which the ovules are attached: this column is never in connexion with the style. Ovules 6 to 10 in each division, about 3-series, erect when the column is very short, but also horizontal and pendent when it is longer; each ovule is attached by a cupuliform tubercle to the placenta.

Style simple elongated

Stigma almost obsolete.

Drupe superior very fleshy surrounded by the free persistent calyx.

Pericarpium thin & pergaminaceous, 3-grooved 1-locular, by abortion 1-seeded (rarely 2 or 3) dehiscent by 3 longitudinal lines alternate with the 3 grooves that correspond with the compressed nerve-like incomplete dissepiments. It is transparent & beautifully marked with interrupted red lines radiating from the nerve.

Seed always erect, generally solitary, erect 3-grooved by the pressure of the incomplete dissepiments. oval, hilum basilar, sublateral; somewhat flattened by compression of the withered placenta, about which the abortive ovules remain visible. Arilline thin, light brown, raphe distinct longitudinal, simple running from base to apex where it branches and is lost in the summit of the testa.

Testa thin, membranaceous.

2 internal integuments distinct, extremely thin and transparent.

Albumen white opaline oval fleshy subglobular

Embryo nearly the length of albumen

Radicle cylindrical inferior pointing to the hilum

Cotyledons broad white opaque compressed ovate subcordate, thin and foliaceous nearly twice the length and 6 times the breadth of the radicle, distinctly nerved.

Hummersmith 29. Novem^r 1856.

My dear Doctor Gray

I was very glad to receive your favour of the 20th ult^o and the copy of your "Manual of Botany" for which I beg to offer you my best thanks: I find it a great improvement on the former edition and an excellent text-book but the faithful and characteristic drawings of the flowerless plants, which are so ably executed, render the work still more instructive and valuable. Since I last wrote to you I have not been idle. You will see in the part of the Linn. Trans. just published all I have stated in regard to the nature and development of the arillus in plants, an investigation to which I was led by our difference of opinion as to the nature of the outer covering of the seed in Magnoliaceae, and you will notice the curious phenomena I have described, in connexion with the development of the Raphe and my detection of the frequent occurrence of a crustaceous arillus in plants, which tunic has hitherto been considered to be the testa, understanding by that term only the tunic resulting from the growth of the primine. Similar inquiries have been followed up in other papers I have now in hand - one on the Winteraceae/ Illiciaceae, where I ~~was~~ have been able to verify, what I have long more than suspected, that Canella is closely allied to Drimys and in no way related to Platonia: it agrees with the former in the structure of its seed, which is a very characteristic feature: in the Winteraceae, the outer tunic, generally taken to be the testa, is also an arillus. - I have likewise drawn up my notes on the Styraceae, and have brought fresh evidence in support of my views respecting this family, acknowledging that in your notes on Vavoea you are right in your structures in some instances, but mistaken in others: my earlier notions had been framed principally on my examination of the So. Amer. species of Styrax / for which I restore Cavanilles' genus *Strigilia* / which differ in many respects from *Styrax* proper to which I suspect your No. Amer. species belong and where the Corolla is more or less imbricated in ostivation and as occurs also in *Halesia* & *Sterrostryax*.

a conspicuous feature occurs in most of the genera of this family, where the outer coating of the seed is a hard crustaceous arillus and where an araneiform raphe consisting of innumerable fine loose spiral threads that interpose as a cottony web between the arillus and the true testa or intermediate coating. *Halesia* however is an exception as it has no such arillus - it has merely a simple testa and a common raphe. Thanks for your kindness I have been able to comprehend the entire structure of this genus: at an early stage its ovary quite corresponds with the well marked structure of *Styrax*, but it subsequently undergoes a singular metamorphosis which I have been able to trace in its growth in a living state: - by its peculiar mode of growth its pseudo-cells are brought into the position observed in the ripe fruit and where becoming covered over by ossified deposits they are converted into perfect cells. Latterly I have been working on *Rhamnaceae* principally with the view of defining the genera of the tribe *Colletieae* to which my notes and drawings made 30 years ago in Chile serve to contribute especially on *Retanilla*, *Discaria*, *Colletia*, *Ochetophila*, *Trevoa* & *Talquenea* which last genus has been confounded with the preceding by all botanists, even by Gillies who ought not to have done so as he had seen my original notes. In the seeds of all the *Colletieae* we find a hard osseous arillus as in *Winteracea*, the raphe being perfectly free from it and adhering to the intermediate tunic. There are some phenomena however, connected with the structure of all the *Rhamnaceae* that are very curious, showing there has existed a motion of the embryo-sac together with the embryo, independent of that of the coats of the ovule. This you will partly glean from your own excellent analyses of this order in your "Genera". You know that in *Rhamnus* and *Berberis* alone the raphe is seen on the dorsal face of the seed, as in *Euonymus*: in all the other genera it is found always on its lateral margin.

Mr Bennett has established, if not on the authority, certainly with the sanction of Mr Brown that at first in all the *Rhamnaceae*, the raphe in regard to the ovule is in the ordinary position, i.e. - on the ventral face

and of course opposite one of the cotyledonary faces - but that by the torsion of the funicular support, the raphe afterwards becomes either dorsal or lateral but he has forgotten and no one has since reflected, that if this were simply the result of torsion ^{of the funicle} alone, the embryo would be twisted round as much as the coatings of the ovule; ~~but~~ ^{however} for we do not find this to be the case, because in most of the genera of the *Rhamnaceae*, the raphe is not opposite one of the cotyledons but opposite to its ~~axis~~ ^{its commissures}, showing that there is a motion of the embryo-sac independent of that of its coatings in these cases amounting to $\frac{1}{4}$ of a gyration, analogous to what I have shown to exist in *Stemonurus Cucurbitaceae* only that there the revolution is in a vertical direction - here it is ^{also} in a horizontal sense. This phenomenon is not the only one, for I have just been examining the seed of a *Zizyphus* which has precisely such a crustaceous arillus as in the *Cucurbitaceae* the testa is of smaller diameter with loose cellular tissue interposing between it and the arillus which renders it easy of separation; it is a spongy compound membrane, pointed at its base into a thread terminating in the hilum, on each lateral margin is a projecting membranaceous sheath ~~starting~~ which starting from the hilum on one side, goes all round its periphery, crosses the Chalaza, and returns along the opposite side to the hilum; this sheath is adherent to the testa, is in no way connected with the arillus and is filled with fine spiral vessels seen nowhere else, and is therefore manifestly the ~~raphe~~ ^{the} raphe - there is no scar, no mark indicative of any former connexion with the summit of the arillus and the chalaza, which there would necessarily be, if the former had its origin in one of the coats of the ovule: from this circumstance and the position of the raphe, we have strong presumptive evidence, that it is of extraneous and posterior formation there is an adhesion over the area of the large chalaza between the testa and inner tegmen, but otherwise these 2 tunics are free from one another, intervening loose tissue existing between them; the tegmen is shorter than the testa, is more contracted below, and terminates in a sort of suspensor. You will thus perceive here, nearly a perfect repetition of the phenomena I have described as existing

in the Cucurbitaceae and a satisfactory corroboration of my former observations. — I have also made much progress in my investigation of the Clusiaceae, a long and difficult task, of which much yet remains to be completed. I availed myself of the opportunity on my return from Switzerland lately, of examining the collections of the Paris Museum to which I devoted a week, but I must return there and shall probably visit Berlin & Vienna with the same object, as I am anxious to complete as far as I am able. our knowledge of this interesting family, which has been so imperfectly described and so little understood. I have also to work up the different groups that must be excluded from it — *Moronea* de *Calophyllum* de *Alamnia* de — *Matonia* and some others.

The *Planta Tendariana* which you say you kindly sent me last spring have never come to hand. — I have parts 1 & 2 of the *Planta Wrightiana* — 1 part of *Planta nova Thurberiana*, and your "Notes on *Davoa*" which long ^{ago} I thankfully acknowledged. We returned all much improved in health from our annual run on the Continent this time we visited the falls of the Tosa, and explored the Southern flanks of Monte Rosa and its gigantic glaciers — for 2 years previously we had examined all the wonders on its northern side, in the valleys of Garmath and Saas — certainly the most magnificent alpine scenery I have ever seen. As you say nothing of the health of Mr Gray or yourself, I conclude you ~~are~~ both well. Mr Miers and my daughter most cordially join with me in kindest regards to her and you. Believe me always,

very truly yours John Miers

2 Dec. — I have examined with the greatest care the seeds of *Petanilla Trevoa* and other Colleticeae, as well as that of *Reissckia*, which all have a thick crustaceous arillus — the testa though separable adheres closely to it, but the inner integument is free from the latter, its chataya being connected with it only by a single point and this latter tunie is again connected by a suspensor with the hilum. Here the raphe runs all round the periphery of the seed, not on one side only as always supposed; it assumes the form of a dark line upon the adherent testa and is filled with spiral vessels which are seen nowhere else, and this same phenomenon exists in all the instances I have examined. That the raphe really belongs to the intermediate tunie of the testa is rendered evident by the case of *Zizyphus* where it is quite unconnected with the arillus. Is this structure peculiar to the cases mentioned, or is it the prevailing rule in the Rhamnaceae — this we have yet to discover. You will excuse this long letter but I thought you would like to have the earliest notice of these interesting facts which I have yet communicated

Hammermith 30. June 1857

My dear Doctor Gray

I ought long before this to have acknowledged your welcome letter of 23rd March last, but I am always a lazy correspondent a fault which I feel it difficult to conquer. I cordially reciprocate in all the kind feelings you express and if I differ widely from you in my notion of the nature and origin of seminal tunics that can in no way lessen my appreciation of the value of your friendship. I think however we are coming closer to a solution of that knotty point. Your valuable investigation into the growth of the ovule led you to give up one position in regard to the bony nature of the bony shell of the seed of Magnolia which you acknowledged to be testa and not tegmen, but this rendered it necessary that you should account for the origin of its fleshy covering which you considered to be merely its epidermidal coating. This happy thought would have made your case unanswerable but for one point which you in common with all other botanists have overlooked, and that very point is my strong arm, as it shows that your solution is only hypothesis and I think you will perceive before very long, an untenable one. Perhaps after all, we are nearer in accord than we imagine and that our difference is more in words than in facts. Putting aside for the moment the question of the origin of the outer tunic, it appears to me we have evidence that it must have grown over and with the raphe subsequent to the act of inversion of the ovule, as in no case could the raphe have penetrated the primine (or testa) at any other than the chalazal point: but what is the fact - we find the nourishing vessels enter this outer tunic at the very opposite extremity and to have become so circumstanced this tunic must have been moulded round it. If you look attentively at any inverted seed, where a raphe is visible, especially a branching raphe subdivided over its surface, you will find the ramifications begin to spread themselves, not from the original base of the ovule, as they ought to do upon your theory, but from the opposite or hilar extremity and you will see it imbedded in the outer integument which you call the epidermis of the testa: if that integument be nothing more than a development of the outer tunic of the ovule; then I contend, that it is against

the laws of nature to assume that the raphe so circumstanced could possibly have penetrated the primine at that point: botanists have too carelessly jumped over this consideration, and have satisfied themselves with the theory so beautifully illustrated by Mirbel without noticing this stumbling block to its correctness. Perhaps you will say that this external covering is modified by the subsequent production of an epidermis which thus embodies the spreading vessels of the raphe within the tissue of the primine and if you do, you grant all that I contend for - viz - that whatever be its nature or origin, it must be of subsequent growth and therefore not a simple development of the primine of the ovule. Call it if you please epidermis or even testa, but let it not in consequence be understood to be the original epidermis of the primine or a mere development of that primary tunic for such it cannot be, judging from the facts as we find them. Gaertner, in whose time little was known of the origin of the different coats of the seed, was evidently impressed with the notion of its subsequent growth, even in cases where it is adherent to the testa: he also adopts for it the same name that you do (epidermis) and classes it with the arillus, both of which he considers "additional integuments superadded to the testa". (Intro. 135-136). — Take again the instance of *Styrax*, where the external coating instead of being fleshy is a bony arillus, when you will see that the tubular sheathing enclosing the raphe - as it issues from the torus or placenta, enters the base of this coating, at the diametrically opposite point to that of the Chalaza) pierces it diagonally and reappears in an entire state upon the inner surface of this bony tunic: here it becomes ramified, and rapidly divided into innumerable delicate spiral threads that form a cottony web intervening between the bony tunic and the true testa: now, applying your rule this bony tunic would be epidermis: if such were the case and it were simply a development of the epidermis of the primine, it follows that the vessels of the raphe must have, at some period, cast off their connexion and continuity with the Chalaza and have pierced the epidermis of the primine at the original apex of the ovule, in order to gain the position we observe it occupy in the ripe seed - and this I contend is a manifest impossibility. You can see distinctly in *Styrax* where the bony coating has moulded itself round the funicular end of the sheath of the raphe and has itself become ossified into a bony tube with a hollow passage in the centre, for the passage of the spiral vessels: These

facts are most palpable. Although I have combated your arguments in regard to *Styrax*, I think you will be interested in my memoir on that family long since ready - It contains also many novel facts and shows some developments that occur in *Halasia* that are exceedingly curious. — I have collected a great mass of evidence from different quarters all tending to prove the opinion I have avowed, relative to the arilliform nature of the outer coat of many seeds, hitherto considered as testa, and perhaps one of the most instructive proofs is afforded by the seed of *Larotizabala*. Here the outer coating, called its testa, cannot be so; it is a black chartaceous crust of a reniform shape with a small round hilum on the ventral sinus. I send you a longitudinal and transverse section showing the capacious hollow within the hilar spot, filled with fleshy matter which you will see is an emanation or extension of the funicle, which has undergone a cyclotrical inversion, for the cord of the raphe enters the hilar aperture of the outer coating, making nearly a perispherical circuit round the fleshy mass, and attaching itself at the same time to the corresponding portion of the inner membranaceous covering of the albumen (the true testa) and loses itself at the point where the minute embryo is imbedded, without once coming into contact with the outer coating. If that coating be the epidermis of the testa how could the funicle and placental mass get inside of it? — indeed it is clear that it must have been formed at a period after the ovule as well as its funicle had moulded themselves into the shape displayed in the seed, and it can have derived its origin from no other source than from the placenta or from its funicular emanation. — I may perhaps be considered too mechanical in my views upon the subject - if so it is the fault of my education in order however to show you the progress of my conviction I enclose you a sketch of the model I made to demonstrate this matter, and would recommend you to have one made like it in order to consider the point well, when you will soon convince yourself of the infallible result of nature's laws that the Raphe however it may be seen attached, or agglutinated to the coats of the ovule, it must necessarily in every case of its inversion remain outside of the original primine and must remain so in regard to it, during the progress of the

growth of the ovule into a seed. — All botanists in treating upon the ovule have almost ignored the matter of the Raphe, but the day will come when so important an element in the structure of seeds will receive the attention it merits. See how completely it has been overlooked and misunderstood in Rhamnaceae yet how singular its development, showing that changes must have taken place within the ovule, the nature of which is yet quite unknown — we have indeed yet much to learn of such matters, and our long received theories must undergo great modifications. I think I mentioned to you that I had prepared a monograph of the Colleticeae where I show many curious facts relative to the seminal structure of the whole order. I have also worked out the Winteraceae and the Banellaceae — the So. Amer. Elaeocarpeae, which can hardly exist as a separate tribe of the Tiliaceae. I hardly know when I shall finish up Calyceraceae Guttiferae Menispermaceae Olacaceae &c — for which I have such abundance of materials ready: I have been for some time devoting myself almost exclusively to get out the 2nd Vol. of my Illustrations now so shamefully delayed and have been obliged to lithograph the plates myself having failed in ^{most of} the professional assistance I obtained and been obliged to do all over again — this is a kind of labour I am ill disposed to give my time to. I must in the same manner bring up the ancients of my promised "Contributions" which serve to illustrate my monograph on the Scacinaeae. — I have lately been examining the Malosma of Nuttall, referred erroneously to my Lithraea, from which it is essentially different. As perhaps there is no family where the characters of the genera are so ill defined and the seminal structure so little understood, I have thought it might be useful to publish what I have observed in the So. Amer. species of the Anacardiaceae, and these I will illustrate by numerous analyses. Here again we fall upon the important feature of the Raphe and upon the results to which an examination of its position leads us. The bony or corneous nut in Lithraea, Davana, Malosma, Sekin us &c cannot be, as I had imagined it, the endocarp of the fruit — it is evidently an adventitious coating of the seed, and no doubt of arilliform origin. I fancy you will smile when I say so, and will declare I am riding a hobby, but we must not too hastily disregard facts, because they are at variance with all our preconceived notions, and with our settled conclusions: the evidence on this point is curious and positive.

I am desirous of seeing your No. Amer. genus *Styphonia*: from the published description I see no single character at variance with *Malosma*. What is really the genus *Rhus*? The admirable figures in your "Genera" pl. 159 & 160 - show a very dissimilar structure from any the No. Amer. form (*R. juglandifolia* HBK) and different from any I have met with in the *Anacardiaceae*, for I have not yet seen a free funicle unattended by a complete tunic attached to it, rising from the lower part (and then never from the base itself) and rising to the summit, there holding the ovule in suspension, a character attributed to the whole family in all botanical works. The nearest resemblance to such a structure I have noticed in *Anacardium* but I have found what is there figured for a free funicle is a prominent placenta parietally attached by its edge till it reaches nearly the summit of the cell and from its termination the ovule is suspended by a short funicle: you will easily see the proof of this in the ripe seeds of the common *Cajou* nut: the distinction is important: there are many novel points of structure in this family which appear to me of extreme interest especially the facts upon which I divide the ^{order} ~~family~~ into 2 well marked tribes.

I have here carried this letter to a much greater length than I intended which I must beg you to excuse. - You do not hold out any prospect of our meeting this autumn - in what direction we shall this year ramble is not yet determined - it is probable I should like to visit Berlin again and certainly spend some time at the Jardin des Plantes in Paris, to work out some points I have in view. Mrs. Allen and my daughter desire to join with me in very cordial regards to Mr. Gray and yourself. Believe me to be always very truly Yours

John Allen

3 July - I will add a few lines to say that I shall have ere long something to remark about *Myrsinaceae*, and give my observations on the carpological structure in that family, which has not been well understood: it offers another exceptional case against our ordinary laws of seminal development. In *Eumyrsineae* you will find the integuments of the seed erect and antitropous but the embryo is transverse and heterotropous without the least trace of Raphe, the Chalazae being found at the basal hilum: the albumen, when the integuments are removed, is perrious and open at the

radicular extremity: the integuments, in drying, show a ~~de~~ small depression over this vacuity which has been erroneously considered as the micropyle, but they there show no vestige of any foraminal aperture. The outermore lax tunic, called by DeCandolle an indusiate membrane of the placenta, (by you it would be called epidermis), is simply a true arillus, though membranaceous and entire. In the *Theophrastea* (I speak of *Clavija*) the seeds perfected are 8 - all attached by threads to a globular free placenta upon a long stipe; the seeds ~~are~~ do not stand erect upon a basal hilum, as in the *Eumyroinae*, but are attached by their ventral face; the embryo is parallel with the axis of the fruit the radicular ^{end} pointing downwards, ~~as in the other tribe~~, as in the other tribe, but there is no vestige of any hilum upon the integuments. The testa of each seed is closely invested by its own fleshy arillus, in the soft substance of which, the short digitate arms of the funicle disappear, and furnish in this manner the support of the seed, and to crown the anomaly, the whole 8 seeds are again covered by one common involucreal soft fleshy ^{arilliform} membrane, quite entire, which encloses all as by a single envelope!! Here again the embryo is transverse in respect to the point of the support of the seed, but the nourishing vessels do not extend as a raphe, either towards the radicular, or cotyledonary end of the testa, as they ought to do according to theory. The embryonal cavity of the albumen is larger than the embryo itself, which lies loosely within it, ~~some~~ somewhat after the manner I have pointed out in *Strychnos*, *Diospyros*, *Anona*, *Stemonurus* &c. (*Linn. Trans* XXII. 103) where I have suggested that the embryonal cavity is due to the persistence of the embryo-sac, and the extrusion of one of its extremities, which has probably broken away from its usual point of attachment to the integuments, and hence the cause of the anomalous position of the embryo in such cases. In *Eumyroinae* the albumen is absolutely perforate about the radicle - In *Clavija* it is not so, but it is reduced to the thinnest possible degree of attenuation - the embryonal cavity is also lined with a distinct pellicular membrane

My model is formed of a board of this size

This represents an antitropous
ovule - where the Chalazal is
confounded with the hilum and there is
no raphe - the nourishing vessels penetrate
the Chalazal immediately without any
intervening space

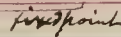
A piece of mahogany board 1/4 inch thick, movable upon
a fixed centre - covered first with yellow paper to represent the Primine
next with blue for the Primine secundine and with white for the Tercine
The elastic Secured band - is fixed into the original base of the ovule (its Chalazal) and
represents the Primine and Tercine
Secundine and Tercine
Tercine or Nucellus
Embryo sac
microphyte
hilum

2 pieces of board covered with
pink paper to represent the
Nucella - shows a groove
between them in which
is placed a band of
India-rubber
coloured scarlet
to represent the
nourishing vessels
it is free to
expand and
contract
as needed.

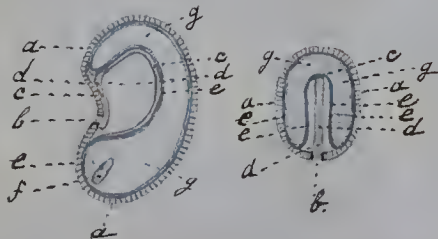
Nucella.

fixed point

and an emanation
from the
Placenta.



Lardizabala.



- a = black chitaceous outer tunic (arillus) - hitherto considered to be the testa of the seed.
- b = hilum or open foramen through which the nourishing vessels pass from the placenta.
- c = Chord of spiral & nourishing vessels ^(raphe) running from the hilum round the large deep hollow vacuity in the middle of the albumen and terminating in a spot near the embryo
- d = fleshy lamellar plate dividing the hollow vacuity into 2 spaces and round the periphery of which the raphe is found to run - it is an emanation from the placenta with which it is connected through the hilum
- e = 2 distinct integuments, membranaceous, the (Testa & Tegmen) that closely invest the albumen, both over its external surface, as over its internal hollow space - they adhere together & to the more external raphe
- f = Embryo - radicle pointing exteriorly - cotyledons directed to the termination of the raphe
- g = corneous albumen.

Hammer Smith 24 June 1858

My dear Doctor Gray

I was very glad to receive your much esteemed favour of the 10th ult. together with the sound lecture you gave me, which I received in the same cordial good feeling with which it was written. You will however perceive in the meanwhile my subsequent communication in the May Number of the Annals, that I have ascertained the source of our difference and that there is now but little variation between us. I fully acknowledge the accuracy of ~~what~~ what you have so ably detailed. Especially since I have myself watched the growth of the ovule in general with a more critical eye and I admit that the fleshy coat, ^{in morphologia} but not the nut, is the growth of the tunic you call the primine, and that the testa in most anatropal seeds is derived from the same source. But with this full concurrence I cannot conscientiously retract my inference from the data upon which my previous argument was founded, for those data were not my own, but such as are to be found in our best elementary works. I therefore still contend that ~~if~~ my fig. 1. p. 277 be an exact representation of the manner in which an anatropal is developed, and which I had been taught to believe, it is perfectly clear to me that any fleshy tunic in which the raphe is imbedded could only be formed by the subsequent extension of the placental sheath of the raphe. ~~As it is clear to me that the data of our most eminent authorities are founded in error, then my inference from them shares the same fate and is also wrong.~~ I feel myself bound to explain to you how this is, and to show you that I have invented nothing nor have I strained any asserted fact. — St. Hilaire / morphol. vég. p. 540 thus details the process of development; the first appearance upon the placenta is a small protuberance, the nucleus of the future ovule, which by degrees becomes covered by 2 cups rising successively from its base / the future primine (secundine) — the so-formed "ovules Anatropes par la courbure graduelle de la base de leur axe se rapprochent peu à peu du cordon ombilical, et après avoir décrit un demi-cercle la rencontrent se soudant avec lui, et la confondent en quelque sorte dans leur substance; souvent le cordon ainsi soudé se montre comme une proéminence extérieure, mais souvent il ne se laisse point apercevoir la partie ainsi soudée porte le nom Raphe." and in order to impress this action more forcibly upon the conviction of his readers St. Hilaire compares this ovule and the relation which the hilum Chalaza and tunics bear towards each other, to a flower inverted by being suddenly bent down close to the base with its pedicel agglutinated to the calyx and corolla: here the corolla represents the outer tunic of the ovule, the Calyx indicates the Chalaza, and the pedicel shows the raphe — just as I have represented it in my fig. 1. A similar explanation is given in Jussieu's admirable work "Cours élémentaire" p. 343, where he defines his meaning by figures: fig. 1. is said to be the nucleus first developed — fig. 2 the same with the primine — fig. 3. the same more advanced with the addition of the secundine — fig. 4. the same inverted. "après avoir fait un demi-tour de révolution" fig. 5. the section of the same when it is explained how "le faisceau vasculaire qui aboutissant à la chalazé la suit dans sa révolution en s'allongeant, et cette prolongement forme le raphe." Lindley / Introd. 180 expresses the same in somewhat different terms: in an anatropal ovule one of its sides grows rapidly while the opposite side does not grow at all, so that the point (foramen) of the ovule is gradually pushed round to the base while correspondingly "The base of the hilum

"is removed from the hilum to the opposite extremity, and when this process is completed the whole of the inside of the ovule is reversed". — Now my own observation upon the growth of the ovule convinces me that these descriptions are altogether erroneous: they show me that it is not the nucleus that is first developed, but the placental sheath, that the nucleus and its proper coat grows out of a channel or depression seen in this sheath and that the margin of this channel scarcely ever changes its position, forming the mouth of the pouch or foramen of the outer tunic subsequently produced by the downward growth of the body of the sheath, while the increment of the nucleus and its proper coat (secundine) takes place entirely upwards at a corresponding rate and thus the outer tunic is formed. It is important to observe that in this development the axis of the ovule scarcely changes its direction, that there is no inversion whatever of the nucleus or of any part of the ovule, nor is there any excessive growth of one side of the ovule, while that of the opposite side remains stationary, for it appears to me in all ordinary cases both sides of the outer pouch grow alike: — there is no agglutination / soudure of the channel of the raphe to the outer tunic simultaneously with the assumed inversion of the ovule. In fine it is easy to account for the position of the raphe in its various forms by the mode of growth I have indicated while under the process of development expounded by St. Hilaire and others it is quite impossible for the Depôts of the Raphe to become distributed in the manner we find them without violating the rules of nature except under the supposition of the extension of the sheath in the manner I had suggested. — It is also worthy of remark that the great man whose loss we have now to deplore, to whom we owe the grand discovery of the changes that occur in the development of the ovule, makes no mention of the turning of the nucleus or its coats during the progress of its anatropal growth. Mirbel who extended Mr Brown's great discovery based his theory of anatropy upon the misconception of the turning of the ovule upon its centre to the extent of half a revolution, while Jussieu St. Hilaire and others in adopting Mirbel's views have given a more explicit but a more erroneous amplification in the terms quoted above, which amplification has since formed the general Creed of botanists. I firmly believed it and without thinking of verifying it by observation, have been led into the error I have committed.

The only point of difference now between us, is whether the bony nut of magnolia as well as its fleshy coat are both produced from the growth of the primine. I cannot realize the idea that it is formed by the deposition of sclerogen upon its inner surface while its outer moiety escapes that operation. Your parallel of the fleshy covering of the Peach singularly coincident with that of Prof. Ascherson appears to me far from analogous: to me it seems as clear as possible that the nut of the almond or peach is a solidification of the entire carpel (not of its endoderm) as is proved by the position of the Chorda pistillaris seen first outside the shell and thence passing its sutural line into the interior, to where the seed is attached — or see it proved also by the presence of the woody fibres in the substance of the shell. To me it appears that the fleshy pericarpial envelope of the fruit is of distinct origin analogous to the pericarpial coating in the fruit of Euphorbiaceae or other multilocular seeds with septiceidial deris coenae where its nature is best demonstrated. See Gaertner Pl. 108-109 &c. where the elastic cocci are produced by the solidification of the component carpels — and where the outer covering is truly pericarpial: if these nuts were formed (as you imagine in the Petal.) by the deposition of solid matter upon the inner lining of the carpels we ought to find a portion of the outer covering between the inflexion of the carpels, but we find the adjacent sides of these nuts perfectly free from any intervening substance and showing by their nerves that each nut is a solidified entire carpel. DeCandolle long ago pointed out the source of these fleshy envelopes in fruits, as originating in a development from the torus — See his figures 1 & 2 Plate 43 in the case of rhipan which he gives in demonstration of his views on this subject (org. vég. II. 40-) and he alludes to the case of *Samia montana* where this coating is almost free. — If I am wrong in attributing to you what you never expressed in relation to *Euonymus*, I will readily acknowledge my error — your words, to which I referred are few for you only say the outer coating is an aril, but your figures appeared to me to express what I stated. I regret having alluded to the subject and for the future will carefully abstain from interfering with the opinions of others and will deal only with facts as I observe them.

I continue the investigation into the very difficult family of the Guttiferae which has now occupied my attention some years for which I am collecting all the information in my power and am now working out the notes made in Paris last year — I have also been endeavouring to bring into some more orderly definition the South American Anacardiaceae — I will not attempt to go beyond that limit but there is much connected with its structure that demands a full investigation — I have now several papers finished and intended for illustration these will come out successively and this bring to mind a fact connected with the Styracaceae that will interest you — I was very anxious last year to examine the fruit of *Styrax* and on my return from the continent ^{in the autumn} ~~last year~~ went to Kew to look if the *Styrax grandiflora* which I had observed in flower had perfected any seed — to my surprise I found upon it the fruit of a *Foetida* — I pointed it out to Mr Smith, who told me that was the original tree upon which Aiton established the species in 1789, which had been brought from Virginia. It has just been in flower — Can this be identical with Pursh's plant? do you know any thing of the fruit of *S. puberulenta* its evident ally? no allusion is made to the seed of either plant in my account I can find. DeCandolle says it has been confounded with *S. lewis* Will. a "species dubia" alluded to *S. grandifolia*. *S. lewis* is also given as a synonym of *S. americanum* evidently a true *Styrax* from the description of the seed. I prefer all the South American species so different in character in the situation of the corolla & structure of its stamens to Cavanilles' genus *Strigilia* — and most of the Indian species which have a similar situation to Loureiro's genus *Cyrtia*. I have something to show respecting the seed coats in the Styracineae as distinguished from those of the *Foetidae* — the structure of the whole family is interesting: of course I maintain my ground, which you combated regarding the absence of any essential relation between that family and the Symplocaceae and I think the evidence I have adduced will place this question beyond doubt.

If I had not received your letter I should certainly have written now to tell you that I had sent you the 2nd volume of my Illustrations which I hope will reach you safely — I was enquiring of Doctor Hooker how I could forward it, when he told me he was sending a parcel to you, & I therefore left it at Kew the end of last month together with another copy for Dr. Torrey. — To him and I conclude also to you, I must offer my best thanks for the very valuable reports of the Botany of the surveying expeditions of Lieut. Burchett, Capt. Pope & Lieut. Whipple with their beautiful illustrations — I had previously received your account of the plants collected in Japan, expedition for which I also return my warm acknowledgements. I have now begun in earnest to get out the plates for my "Contributions" which I will send you as soon as they are ^{thought to me} ~~completed~~ ready. I am shamefully behindhand in this matter which has become an affair of greater ~~importance~~ ^{importance} to me. You will perhaps have some difficulty in deciphering this letter, as I am now obliged to use both hands to guide my pen owing to an infirmity in my right hand from which I am told there is no hope of relief: this loss of power in the 2 last fingers and weakness in the others came on soon after my return from the continent last year and has much disabled me from my delicate work. I am now getting into a better way of managing the hand. You will I am sure deeply regret as we have all done, the great loss we have suffered in the death of Mr Brown, certainly one of the greatest men of the present age and whose celebrity will be immortal — there can be no doubt that to him in an chiefly

indebted for the present advanced state of scientific botany, he having pointed out the path which others have successfully followed: those only who had the pleasure of his acquaintance can know his many rare attainments and the vast extent of his talents.

You do not say anything that may lead to the chance of our meeting you and Mr. Gray this year - be assured we shall be glad of that pleasure when it next takes place - we all desire to be more kindly remembered to him and to you. Believe me always
Very truly yours
John Miers

26 June. - The forthcoming number of the annals will contain the first part of my paper on the Winterae in which you will perceive what I have said on the seed-coats in that family. The second coat there, is analogous to the fleshy tunic of magnolia, but what is the outer brittle coating? is it not arilline? it cannot be analogous to the bony shell of magnolia (viewed in the light in which you regard its origin, with this difference, that in the one case sclerogen is deposited in the endoderm of the primine, and in the other upon its epiderm) - because we see the shell in the winteracea lined with a distinct reticulated membrane that is separable from it, and because we find some layers of large loose cells filled with oily matter lying between that membrane and the fleshy coating which encloses the raphe. This difference of origin is still more palpable in *Lardizabala*, where there is a similar crustaceous ail between which on one side and the fleshy tunic containing the raphe is a large chamber round which the raphe takes an almost ~~spherical~~ ^{conical} circular course. You wish I will give up my crotchety notions about ails & arillines, but how is this possible with such facts staring us in the face. In describing them however, I have tried to avoid any speculations about them and have endeavoured to confine myself as much as possible to the simple details of the several structures - the analyses I have repeated scores of times carefully, always with similar results.

I have just compared the American specimens of *Styrax grandifolium* in Sir W. Hooker's herbarium, and find they do not correspond with Aiton's plant growing at Kew. These American specimens quite agree with a leaf of some flower you sent me in a letter under that name - they are acute (not with a long attenuated point) they are mostly entire, sometimes slightly denticulated (not serrated with a small gland at the end of each tooth) they are canonically tomentose beneath (not smooth & glaucous, sparse pubescence being only distinguishable by a lens). In Aiton's plant the petiole is double the length, the leaves larger - the flowers fully double the size of those of *S. grandifolium* & larger than in *S. officinale* - the anthers of its stamens much shorter in proportion and the parallel lobes widely separated - the ovary half superior with the long style 3-fid at its summit ~~and~~ ^{separated} by slight pressure to the base into 3 distinct styles, in which respects it differs from *H. tetraphora*. The calyx is short, its tube wholly adnate, not tubular and 5-toothed as in *S. grandifolium* - and it is very different from *H. tetraphora* & *H. diptera* and appears to me a very distinct species.

Hammersmith 30 Decem^r 1858

My dear Dr. Gray.

I was pleased to receive your welcome letter of the 19 Oct. which I found at the Linnean Society together with the 2 first sheets of your "Remarks on Rubiaceae": these have greatly interested me and I truly thank you for them. Your notes on *Timonius* brought to my mind similar conclusions I had formed concerning *Glomelia* more than 20 Years ago when I examined several Brazilian species of the latter genus. Although, as you justly observe, estivation is often an accessory feature of value, it is not of so much importance as that of the structure of the ovary & seed, and I quite support your view and differ from W. Bentham who considers the *Guettardae* and *Coffea* as mere divisions of one tribe: the peculiar plug-shaped strophiole which you so accurately describe in the *Guettardae*, certainly offers too strong a character to be mixed up with quite another development. I am glad you confirm or rather anticipated my observations on the centrifugal mode of growth of what is called an anatropal ovule - I mean that it is not due to any inversion of its coats by a one-sided growth, as hitherto taught, and you wonder I did not acknowledge this in my memoir on *Winteraceae*. That paper was partly in type, but put aside for the discussion of the question of the ovule. In my reply to Dr. Hensley which was to have appeared in the June number of the *Annals* I fully acknowledged my previous error in regard to the ~~mode~~ development of the fleshy coat in *Magnolia* & admitted the correctness of your details, though I differed from you in regard to the origin of the bony shell. As Hensley in defending your case, persisted in ascribing a one-sided growth to the anatropal ovule, I referred to his own paper on the ovule of orchids, as proving the contrary. See *Trans. Linn. Soc.* pt. 2. - There, in accordance with his theory, the progressive stages of its growth, ⁱⁿ his figures of the ovule ~~and~~ make it appear that it becomes gradually inverted, but I showed that if we take the same figures, and restore them to their true position - i.e. with the funicle always in contact with the unchangeable point of its placental attachment, we find no inversion at all, and no one-sided increment of its tunics as he avers, for precisely as many cells are there depicted as produced on the opposite side of the primine, as on that of the raphe, the growth being equally downward in the manner I had attempted to prove.

At the same time I contended that in the Almond fruit, its fleshy covering proceeds from a growth of the torus, as long ago shown by DeCandolle, and that the nut is a solidification of the entire carpel, not of its endocarp as argued, this being proved by the position of the raphe and the presence of the vessels and nerves of the original carpellary leaf within its substance. This paper was printed and the proof corrected, when it was withdrawn at the request of Doct^r Francis, who had shown it to Kenfry, and who had taken umbrage at it, though I had been careful to avoid giving cause for this. I have since however availed myself of an opportunity of recurring to some of these points, in 2 notes in my paper on Styracæ, which I expect will appear in the February number of the Annals, and where also I have frankly, and I hope you will say, fairly examined the facts related by you, in your "notes on Vavoea", where you object to my exposition of the structure of that family. — My next Paper will perhaps be a monograph of the Colleticeæ in which some novel points in the structure of their seeds will be detailed, that I find exist also in Rhamnus and Zizyphus and probably in all Rhamnaceæ. It appears to me evident that the position of the raphe does not become dorsal or lateral by the mere twisting of the funicle as Mr Bennett suggested, for I have found it normally so in the ovule as other botanists have ascertained it to be. But the most singular ^{point} to be noticed, is that I have invariably found the raphe in all these cases perispherical. If for example you take the seed of Rhamnus catharticus and carefully remove the outer crustaceous covering (which is an arillus) you will find upon the membranaceous testa immediately beneath it, a very distinct cord running down the ventral face from the apical chalaza to the micropylar base and which is continuous across the chalaza with the ordinary dorsal raphe found in the groove, and you may, by careful dissection, tear both ~~away~~ with a portion of the testa, away from the tegmen, in one continuous cord, for it comes off from the chalaza without rupture. In all the Colleticeæ this perispherical ^{cord} is invariably lateral, as in Zizyphus, and opposite to the margins of the flat cotyledons: in Rhamnus whether the raphe be dorsal or lateral, it is always opposite to the midrib of the cotyledons. In Rhamnus vitellifera, Dene. the arillus, as you have described it in Alphitonia, is membranaceous or subfleshy — in all ordinary cases it is crustaceous, and always pervious by a chink, more or less considerable in size: I find the ventral portion of the raphe, with its visible spiral vessels, developed in the primine of the ovule, as well as on the testa of the seed. These facts will be illustrated by numerous analytical figures, and I am sure it is a subject that will interest you.

My long pending investigation of the Guttiferae goes on slowly but gradually as I can collect fresh materials — I have also brought together many curious facts connected with the development of the ovule and seed in the Anacardiaceæ, the South American species of which I have endeavoured to reduce into order. — Talking about novelties, what do you think of Agardh's new system of arrangement? his views baffle all our previous notions on this head, but his extraordinary associations appear to me quite unsupported by all the rules of true affinities. I have already combated his facts as well as his notions concerning Styracæ, which he places near Tiliaceæ, especially Elæocarpeæ. Aristotelia, which he considers (though without any good reason) as ordinarily distinct from the latter tribe, was carefully investigated by me many years ago, together with other Chilean and Brazilian Elæocarpeæ (which is hardly distinct even as a tribe in Tiliaceæ): these details I may probably some day publish. The plates attached to Agardh's work form a valuable collection of facts showing the different modes in which the ovules are developed. — In regard to the so-called "Styrax grandifolium" which I mentioned as growing in Kew Gardens, I found a few days after I last wrote to you, that it is identical with Halesia diptera — as compared with original American specimens. I notice that its leaves as well as those of H. tetraptera have the points of their teeth furnished with a gland often stipitate, which I do not find in any of the Styracineæ.

Mr Miess and my daughter join with me in kindest remembrance to Mr Gray and yourself and cordially wishing you may both enjoy a happy new year and many succeeding ones. Believe me to be always very truly yours

John Miess

In *P. fimbriata* the segments are thick and fleshy as in my Brazilian *Gymboearpa*, but they have in addition the fimbriated processes upon ^{the} back (not on the margins) of the segments: in *P. lineella* the segments are extremely thin and membranaceous.

What therefore is the distinction between the 2 genera? for the several differences I have pointed out, run into each other through all the species enumerated by W Dentham, and I must confess that I see nothing constant in them, nor anything of sufficient importance to constitute a valid generic distinction.

I would sooner have complied with your request, but your plant reached me a week ago, the day after the arrival of my son, who has come home from Brazil to seek medical advice, so that I have been much occupied and very anxious about him. You will no doubt have received the letter I wrote to you about a month since. With kindest regards to Mrs. Gray believe me always very truly yours
John Myers.

Hammermith

23 Feb'y 1859

My dear Dr Gray

Your return enclosed in this, your pretty little Saurauianaceous plant sent for me to examine. I regret it is too old a state to ascertain its structure with any certainty, for when these small fleshy plants become crumpled by desiccation, they are not easily unravelled. I have endeavoured to disentangle one flower, as you will see, but have not liked to injure your specimens: if you have a younger flower, you will probably be able to determine the form of its segmental divisions. I have always found in these plants, after expansion, the boyane of the pollen grains enter so deeply into the stigmatic tissues, and cement firmly together the

anther cells with the stigma that they soon form an inseparable mass. I fancied I could perceive the peculiar stigmatic appendages in this decayed mass, but of this I cannot be certain: the stamens are certainly those of Cymbocarpa, while the gibbous shape of the fruit and its mode of dehiscence most decidedly correspond with that genus, so that I have not a doubt you have named it correctly. You will observe what I have always found in the Brazilian species, that after impregnation, the border of the perianth becomes shrivelled and breaks off by an irregular line across the point of the insertion of the stamens.

I will however direct your attention to the greater length of the tube, in which respect it resembles that of Ptychomeria of Spruce & Benth. This name was suggested by the former on account of the inflected mode of articulation of its segmental divisions , but it does not differ in this respect from Cymbocarpa and Apteria as I have figured in the latter. It is to be

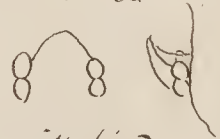
regretted that Mr Benth. has given no figure of his genus, nor has he indicated the differential features separating it from Cymbocarpa. I find no distinction but in the 3-lobed shape of its 3 principal segments, formed by the veins, a character quite wanting in P. cornuta, and only well marked in P. tenella, and his Diplomeria section, but in the latter the internal segments are present as in Cymbocarpa, while in P. tenella they are obsolete: in Cymbocarpa the border expands thus,



in Ptychomeria tenella it is thus;



The stamens are alike in both genera thus.



In the stigma of P. cornuta you will find precisely the same long filiform appendages I have figured in Cymbocarpa: these appendages in other species are less developed, while in P. fimbriata they are so much reduced as to form merely sharp angular prominences.

One of the most curious of these genera will
be dedicated to our friend Kippist whose name
has not yet been honoured in the nomenclature
of botany. At least 17 plates will be required
to illustrate the memoir. I am not certain
whether the Council of the Linnean Soc. will
consent to publish it - if not I must have
recourse to a 4th vol. of the "Contributions" which
I wish to avoid if possible, though I have plenty
of matter in readiness to fill a volume - but
I feel I am getting too old for such a task.

I have not yet published the result of my
examination on which you saw me at work
on a new tribe of Cordiaceae - Auxemneae -
- nor my analysis of the carpal structure
of the Verbenaceae which will require another
arrangement of the family. I have some
observations to make on Scyphidaria
and to point out a peculiar structure in
Scyphus not recognized ^{by Berg} though Martius alludes
to it - it is only seen in the living state, and I
fortunately made a drawing of this while in
Brazil - it will necessitate the severance of
Scyphus and Eschweilera which Berg has
erroneously welded together: I have besides
much other unpublished matter which I cannot
expect will see the light. Pray offer my very
kindest regards to Mr. Gray and believe me
to be always very truly yours John Miers.

84 Addison Road
Kensington
30 March 1871

My dear Doct. Gray

I have not sooner replied to
your kind favour of the 24 Jan^r - last
because I have shrank from the extreme
pain of telling you, in answer to your
kind inquiries that I had the sad misfortune
of losing my good and excellent wife
in September last after a long illness.
I will not dwell on this painful subject,
for which there is no help but resignation,
and though I summon all my courage
I feel the infliction not easy of endurance
notwithstanding that my dear and affectionate
daughter does all she can to alleviate my
distress.

I thank you kindly for your Contribution
on Diapensia Pulemonioides which is a
very valuable accession to our knowledge regarding
them - I was also greatly pleased to have your
Carta, especially as it is so good & pleasant - I
will not fail to send you mine, as you request
by the first opportunity. You ask me if I

received last year the revision of the Engonice
by you and Dart Torrey: this I must answer
in the negative, but I will ask W. Kippist about
it when I see him. You tell me you do not
receive the Linnæan Transactions, and have
not seen my later contributions to them: I
am able with much pleasure to send you
a complete set of my several memoirs,
beginning with the earliest in 1840 if you will
kindly accept them. In less than a month, I hope
to send you my monograph of the Menispermaceæ,
a labour of many years, which will form the
3rd Volume of my "Contributions" - I do not think
you received my 2nd Volume published last
year, but I believe you had the 1st Volume
if not I will send all 3 together, with a set also
for Dart-Torrey. This work you are no doubt
aware is distinct from my Illustr. So. am. pl. of which
only 2 Volumes are published and which I know were
sent to you & Dart-Torrey - my "Contributions"
were slow in making their appearance on account
of the difficulty and the time required for the
execution of the plates, all the work of my own
hands. Will you have the goodness to tell me
how these books will safely reach you, and
whether you have any correspondent in London
who will forward them. I am now completing
a monograph of the Hippocrateaceæ of the new
world, a work which I think will interest you
on account of many points of curious structure

heretofore unobserved; no one has taken the trouble
to investigate the family, or to ascertain the
limits of either its genera or species, the whole
having resolved itself into a state of utter confusion
until our friends at Kew simplified the matter
by amalgamating all into 2 genera in their
Gen. plant. Hippocratea & Salacia, though the
latter genus is not found in So. America. I have
restored and found good characters for the older
genera and others - 17 in all which will be each be
illustrated with ample details. I am quite at
variance with our Kew friends, who reduced the
Hippocrateaceæ to a tribe of Celastraceæ, while
I show these to be altogether distinct, not even
near one another in the natural arrangement -
and though they say (Gen. pl. 358) there is not a
single good discriminating character between
these 2 families, I can show about a dozen
points of structure that serve to separate them.
Among these are some points of structure quite
new and peculiar - there is a group in the family
in which the stigmata are opposite to the stamens
and alternate with the placenta and cells of the
ovary - a fact quite new in Dicotyledones, though
not infrequent in Monocotyledones as Mr Brown
long ago demonstrated, and I can easily
reconcile the different arguments between him
and you in regard to Tarnassia, which in my
view does not come within this category.

steps in the publication of observations
made long ago: I have still a large
amount of material which I cannot
expect to see published - I will however
persevere as long as I am able to do so

Repeating my kindest regards
to you & Mrs. Gray believe me
very truly yours
John Miers

Received
and Wm M.

84 Addison Road
Kensington 4 May 1871.

My dear Doctor Gray.

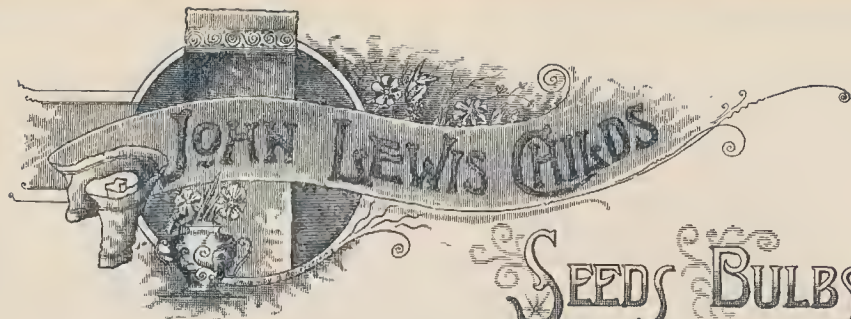
I thank you for your note of
the 18th ult. expressing the sympathy
so kindly felt for me, both by Mrs
Gray and you, under the very sad
loss I have sustained, and which
I still find very difficult to bear:

My daughter unites with me in
cordial thanks for this expression
of your friendship. - I have also
to thank you for your memoir on
Eriogonae, the most recent and a
more careful revision of this interesting
group. I have enquired at the Linnæan
Society for the copy so kindly sent on a
previous occasion, but it is not there:
it may still be remaining at Kew,
where I have not been for nearly a
year - so little have I been from home,

You speak of having only two volumes of mine - these must be my Illustrations of So. Amer. Plants: These were followed by my Contributions to Botany, which I must have omitted to send. I have therefore now three volumes of the latter work to ~~send~~ ^{pass} you forward, which shall be left in Prof. Oliver's care - together with a copy of each of my communications in the Linnean Transactions: among these you see a plate and my remarks on the Chilean plant I collected of Oxytheca. I send these loose in order that my forthcoming memoir of the neogean Hippocrateaceae may be bound up with them in case it should be received by the Linnean Soc. Council. I will also send you a number of the Hort. Soc. Journ. containing a communication which you probably have not seen, upon some Brazilian Bignoniaceae: I had long studied this family and had begun to describe some of the genera, as you will

find in my 'Contributions', which I intended to follow up by successive description of the genera under my new arrangement; but this I gave up when I found Dr. Seemann had undertaken a similar task: the latter however never persevered continuously and I thought I was absolved, when I learned that M. Bureau had accepted the offer of completing a monograph of the American Bignoniaceae for Von Martius' Flore Brasiliensis. Accordingly I sent him my specimens, all my notes, and analytical drawings - in fact all the knowledge I had obtained by long and careful observation concerning the family, which he was very glad to obtain, but I have not heard from him for 2 years or more, and do not know whether he has completed his task.

I enclose my Carte in this, as you requested: you will see from the date at the back, that I ought to be thankful at this age to be able to proceed by slow and careful



SEEDS BULBS & PLANTS

Dictated by _____

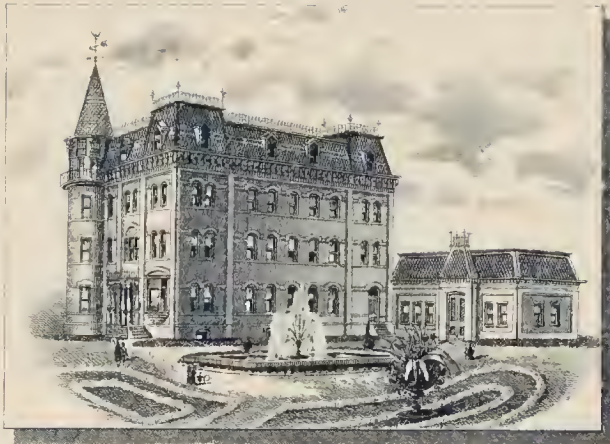
FLORAL PARK N.Y. Nov 1 1889

Dear Sir

The enclosed flower was received from a customer, said to be a wild flower, I cannot ^{now} give you the name of the place where it was found, Can you give me the name of it? by so doing you will oblige

I can get the locality by writing to the one who sent it in.

Yours truly
E. Miller



Dictated by

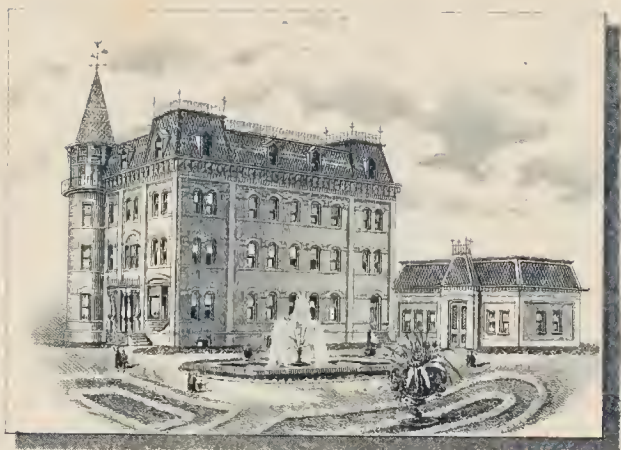
John Lewis Childs, SEEDS, BULBS & PLANTS.



Floral Park, N.Y. Sept 7 1891

Dear Sir

Yours at hand this morning
Thanks for your naming the cinnam. I
send flowers of another we bought for
Amabile. I would send leaf but haven't
any in hand. The bulb from which this
was cut is club shape 18 inches long & about
4-5 inches through at base. Mr Pike thinks
it the same as the one he has which
came from China. I am sorry to
trouble you but hope I can return
the favor. I hope Mr Cameron will not
go to any trouble to get the Mentha.
Yours truly
E. L. Miller



BEATTY & VOTTELER LITH N.Y.

Dictated by

John Lewis Childs,

SEEDS, BULBS & PLANTS.

Floral Park, N.Y. Oct 6 1891

Dear Sir

I send you by this mail
a box containing a little *Oenothera*
from Mexico. Will you kindly give
the name or species. It is a constant
bloomer winter and summer.

Yours truly
E. Childs

P. S.

I send in another box specimen of a
Solanum which I would be glad to have
you name for me.

Yours E. Childs

Solanum atro-purpureum
from Brazil

McLellan's La.
Aug 11 '90

Dear Sir,

I have found one plant
that has every characteristic of
an *Eupatorium perfoliatum*, save
that the leaves are in whorls of
threes, connate perfoliate, instead
of opposite. There are other
E. perfoliatum around it, but
no other *Eupatoriums*. Does
such a freak sometimes happen?

Yours truly

J. H. Mills

Sereno Watson

Cambridge, Mass.

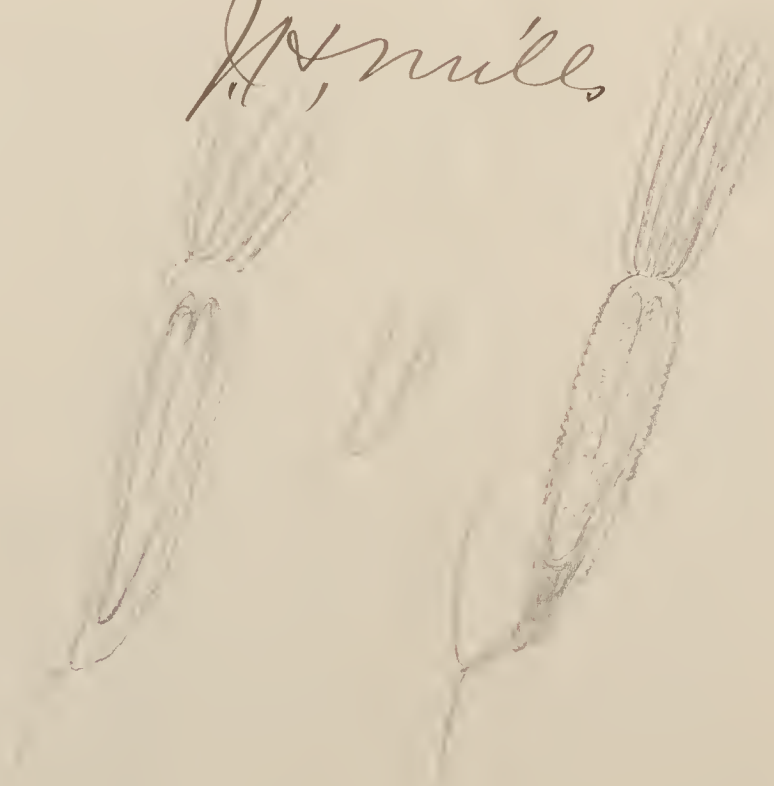
Mr Pleasant, Ia.

Sept - 16 '90

Dear Sir. Gray's Botany under the
head of Onagraceae says "Seeds anetropous."
I send some seed pods of *Epilobium coloratum*.
The seeds seem to me to be orthotropous. I
would like your decision.

Yours truly

S. W. Muller



Millersburg, Pa.
Sereno Watson, Dec 3 '91
Cambridge Mass.

Dear Sir. Could you
get me The Botanical name of
"Dumble weed." It grows on prairie
land, newly broken, is three or four
feet tall, the branches form a loose
light - globular mass of about the
same diameter. The wind rolls
it into fence corners and hollows.
None near here, but I remember
to have seen it in the north
of the state.

Yours truly

J. H. Mills

Mt Pleasant, Ia,

Apr 10 '91

Seamus Watson.

Cambridge, Mass.

Dear Sir. Is not

the *indica* *camptocorus* *rhizophyllus*?

Yours truly

J. H. Mills

I found a *Trillium divale* with several young
tubers growing from the root stock of a flower
in bloom. The books do not mention whether it is
common. The infant tubers were spherical.

W. Pleasant La.

Aug 7 '91

My Dear Mr. Watson.

Polygonum lapathifolium?

Pistils 2-3 parted. Stamens 6-7.
Achene lenticular with concave sides
or concave triangular.

Yours truly
J. H. Mills

Amaranthus retroflexus?

Leaves are peculiar,

My herbarium of the Genus
Euphorbia contains 87 species so
far, represented by 254 individ-
uals, the bulk of which are from
the collections of Dr. Edw. Palmer, Dr. Geo
Vasey, and Messrs. Perrichon, Mohr,
Curtiss, Orcutt, McJones, Lemmon,
Greene, Pringle and Addison Brown (balled
plants).

I have beside about 200 mss.
letters from U.S. and Mex. botanists
noting the sp. in their localities, these
to aid in geographic distribution of
sp.

As literature I have DeCandolle
Prod. Bot Mex Bound. Bot. Calif. Jour
Contrib. XIII. XIV. Boissier's Icones Euph. &
minor works.

Thus much for a ground work.

CHAS F. MILLSPAUGH, M.D.
HAHNEMANNIAN.

Haverly, N.Y. Oct 19th 1888

My Dear Sir:-

I enclose you a violet that has both puzzled and interested me. It grows profusely upon an unusually overflooded meadow in Cayuta Creek near here. This individual is one of the largest found, the usual growth is but little over one inch. My sp. striata in other localities are generally from 4'-8' high.

I have no literature of the revised *Violas*.

The character of the two bracts opposite on the pedicel is in none of the violets in my collection. If it proves new please remember the pride of an amateur in nominating it.

Yours Very Sincerely
C. F. Millspaugh

CHAS F. MILLSPAUGH, M.D.
HAHNEMANNIAN.

Waverly, Mass. Dec 15th 1888

Mr Lorenzo Watson

Dear Sir:-

I have, for two years, been studying on the genus Euphorbia, with a view to publishing a monograph upon the U.S. and Mexican species, this I intend to illustrate with Photo-Micrographs of the seeds. I am endeavoring to make a really valuable work in this, my favorite genus. Can Cambridge help me out any with duplicates; my collection is already large but not complete. Can you as a personal favor furnish me with either, some "debris" from, or duplicates of, your species; that I may have authoritative data on them? viz. E. Plummerae; E. polycarpa, var vestita; E. Rattani; and E. lineata.

Species sent to you for identification, and there must be many, I would be glad to work out for you, thus saving you the use of much valuable time.

I am not playing with this genus, but have my whole soul in it.

Yours Sincerely
Chas F. Millspaugh

P.S. Who is arranging Euphorbia for the
new edition of the flora of the U.S.

CHAS F. MILLSPAUGH, M.D.
HAHNEMANNIAN.

Waverly, N.Y. Dec 17th 1888

Mr Lorenzo Hutton
Dear Sir:-

In my petition to you of the 15th inst. in
wh. I asked as a special favor, that you might send me, either
authentic duplicates, or some debris containing cuts of your sp.
in the genus *Euphorbia*. I should have included the following:

<i>E. gracilissima.</i>		collected in S. W. Chihuahua by Palmer in 1885
<i>E. plicata</i>		
<i>E. subrotundifolia</i>		
<i>E. retroscabra</i>		
<i>E. Guadalupeana</i>	"	at Rio Blanco Mex " " 1886
<i>E. tomentulosa</i>	"	in L. Calif. by Orcutt " 1886
<i>E. uniglandulosa</i>	"	in Mapula Tinto near Chihuahua by Pringle.

Are the Mex. Bound. Sp. with you, or at the
Herb. Columbia New York?

Yours Very Sincerely
C. F. Millspaugh.

CHAS F. MILLSPAUGH, M.D.
HAHNEMANNIAN.

Waverly, N.Y. Dec 18th 1888

Mr Erno Hatson

Dear Sir:- Your favor in answer to my request for duplicates & in the genus *Euphorbia*, does not in the least discourage me, though I had indeed hoped that Cambridge would have a few spare duplicates. I fully realize the labor I shall have to perform before my work in the genus can assume authoritative shape and have planned, that when my acquaintance and study in the genus, shall have become sufficient to spend some time, not only at St Louis with the Englemann collection, but also at Washington, Cambridge, and in Europe with those of Boissier and others. In the mean time I am working to get together material for comparison at those Herbs.

Accept my thanks for the packets you kindly send pointing to *Plummeria*, *Rattau*, and *Polycarpa v. vestita*.

My work is earnest and though it may take a number of years, is to be completed nevertheless, if I live.

Sincerely Yours.

C. F. Millspaugh

CHAS F. MILLSPAUGH, M.D.
HAHNEMANNIAN.

Harvey, N.Y. May 18th 1886.

Mr Erno Hutson

My Dear Sir:-

Please accept my sincere thanks for the "Contributions xvi" which contain much of interest to me in my favorite order Euphorbiaceae.

I have had the pleasure of studying Mr. C. G. Pringle's Mexican collection, in this order, of 1888. I found in it your beautiful *Euphorbia Guadalupeana* (Contrib xiv 449); your *radialis* (ibid 450); *Orton ciliato-glandulosus* (ibid 450); *Stillingia Delavensis* with the same characters that you mention for *Palmeri* n. 73. Your *Euphorbia sub-rundiforme* Contrib xiii 439. and a new species which I have named *E. Montereyi*, together with others of lesser interest.

I was compelled to work out all these Euphorbiaceae upon the descriptions in your Contributions xiii, xiv, and xvi. I have not your xv, which I deplore, "Bot. New Bound Surrey" and D.P. *Prodr. xiv* containing *Wissler's* and *Müllers* names in the order. Having no species for reference in any case. I hope therefore that no errors have crept into my determinations.

I greatly regret my being so far from the larger herbaria.

Yours Very Sincerely:-
C. F. Millspaugh.

CHAS F. MILLSPAUGH. M.D.

Waverly, N.Y.

Oct 21. 1889

My Dear Sir:-

In nominating the collection of Euphorbiaceae, made by Mr. Brandegee in Lower California this year. I find a specimen of E. magdalenae Benth., upon which in place of involucre in many instances I find such growths as I enclose you. They look like malformed involucral cups, I never have noted such before. Will you kindly express an opinion.

Sincerely Yours

C. F. Millspaugh.



Nov 5th 1889.

Mr. Simeon Watson

My Dear Sir:-

Accept my thanks for
the "Contributions" and fragment of E.
intermixta so kindly sent me.

I have now completed
the 44 species collected this year by Mr.
T. S. Brandegee in Lower California, and
found among them your peculiar Sebasti-
ania (?) bilocularis, also your E. tomentulosa,
petrina, polycarpa & vestita, Bernardia myricae-fol.,
and Stillingia linearifolia. This collection

yielded one quarter % new species as follows.
Euphorbia Purissimana, Brandegei, conjuncta,
and Comanducana; Phyllanthus Brandegei,
and ciliato-glandulosus; Ayton Maydalenæ;
Argythamnia Brandegei; Bernardia Brandegei.
(and Euphorbia involuta).

Yours Very Sincerely.
C. M. Spang.

Harvard Medical School
Boston, Jan. 15th 1890.

Dear Sir: —

I enclose a circular of
the Elizabeth Thompson Science
Fund, you can see that it
is necessary if you wish a
grant to apply for a specific
sum for a specific purpose.
It is improbable that we
could make you any grant
before Dec. 1890, but it would
be perfectly suitable for you
to make application now or
later.

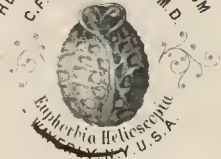
Yours very truly,
Charles S. Minot
Sec.

(18)

To Prof. Berens Watson
Botanic Gardens,
Cambridge, Mass.

Minot

HERBARIUM EUPHORBIARUM
C.F. MILLSPAUGH, M.D.



Worgantown N.H. Oct. 19. 1891

My Dear Sir:-

I am not in condition since
a severe attack of sickness to compare
specimens and thank you heartily for your
favor pointing out the resemblance of *E-*
trallii to *E- angusta* which it really
is. I had only a little scrap of *angusta*
heretofore for identification. I stop the
publication of *trallii*

Very sincerely yours

C.F. Millspaugh

BOTANICAL DEPARTMENT

West Virginia

Agricultural Experiment Station.

C. F. MILLSPAUGH, M. D.,
BOTANIST AND MICROSCOPIST.

Morgantown, W. Va. July. 27. 91

Dear Sir:-

I am preparing to publish this coming winter as complete a preliminary list of the Flora of West Virginia as possible. Can you assist me in giving me such bibliography as you conveniently can upon publications concerning the Flora of this region and the names of such Botanists as you can recall who have worked here.

In the manual you credit W. Va. with *Arceuthobium flavum*, Ait.; var. *purpurascens*, Gray. to whom should I credit the species in this State?

Should it be necessary to put an assistant or clerk upon the references, I would willingly pay for the time thus expended in my behalf.

Hoping that you can help me out in this matter I remain

Yours Very Sincerely
C. F. Millspaugh

Willoughby

West Virginia University.

WEST VIRGINIA

AGRICULTURAL EXPERIMENT STATION.

JOHN A. MYERS, DIRECTOR.

Morgantown, W. Va., July 18th 1891.

My Dear Sir:-

Several years ago Dr. Gray took a botanical trip through the mountains of West Virginia. Do you know whether he published notes of the trip, and where? If the notes were published, or are in any compilation of his notes, essays &c. please consider this an order for a copy if for sale at Harvard, and can be procured without too much trouble to you

Yours Sincerely
P. F. Millspaugh

Amsterdam
Mars 19.

Amsterdam 15 février
1852

- Le travail était terminé, je
repris mes Fiches, pour publier
une monographie complète. L.
Poiré avec quelques matériaux pour
cette notice, m'a été fort utile
grand plaisir. Par suite les par-
ties austral de votre énorme
projet produisant Nam et Paité
espèce de cette famille.

Avec les sentiments de haute
estime et d'affection

Sincère

Votre très dévoué

F. A. M. Meunier

Je ne possède pas encore Tony &
à Graydon of North America.

Mon cher collègue !

J'aurais dû depuis long temps répondre
à votre aimable lettre d'octobre
dernier et vous offrir mes remerciements
bien sincères pour le magnifique
envoi de plantes et de livres,
qui me sont très parvenus. Mais
j'ai eu tant d'occupations par l'orga-
nisation de l'Académie des Sciences
qui vient d'être fondée ici pour les
plantes la première classe (physique)
de l'Institut - que le temps me
manquait de choisir pour vous faire
des doubles les témoins de ma
reconnaissance ! Maintenant
une bonne occasion vient d'être pré-
sentée pour un envoi à l'étranger,

et je ne l'exprime d'en pro-
fiter, pour vous dire que
j'estime beaucoup votre correspon-
dance, que je sera très-haieuse
si vous voudriez la continuer à
l'avenir, que j'ai en la journée
la plus heureuse en ouvrant
le carton avec cette belle collection
des plantes intéressantes et
des livres magnifiques! J'ai
admiré votre bonne activité
et le talent d'observation et de
plume manifesté dans vos
bons livres ouvrages!

Maintenant vous ne devez pas
encore des plantes sèches. Vous
en avez prochainement une
bonne quantité! Seulement

quelques livres ou herbiers
devient son témoignage tra-
gratuit.

Je m'occupe dans ce moment avec
les Vigier, Astruc, Artès,
de Flor Bonifacio de ill.,
de Martin, qui paraissent
encore pendant le winter de
cette année, si les événements
politiques ne l'empêchent
par. Le gouvernement baron
soutient de son travail les arti-
fices de M. Martin. Tout
va bien, si la guerre ne soit pas
troublée. Mais en jetant un
coup d'oeil à la France, ce
peut malheureusement son la des-
potisme de Napoléon, on doit
s'inquiéter un peu à cet égard,

Amsterdam 4 Nov 1852

Mon cher collègue!

A la fin j'ai trouvé le temps, de faire un choix de mes doubles et je viens d'expédier un cartou, contenant des plantes de Java et de Surinam et quelques peu d'autres localités. J'espère de tout mon cœur que Vous y trouverez quelque chose digne de votre intérêt et utile pour vos travaux scientifiques. — Je dois remarquer que ces exemplaires ne sont pas arrangés systématiquement, et sont trouvés dans la même poche des plantes de différentes localités. — J'ai joint quelques brochures et en même temps quelques exemplaires de mes *Illustr. Pap.* que Vous possédez déjà. Peut-être Vous trouverez occasion d'en distribuer parmi vos amis.

À mon absence d'ici, pendant le concert de Noël, j'ai eu une visite d'un de vos amis, qui alors m'a remis votre lettre d'introduction. Je le regrette beaucoup que mon absence m'a privé de faire sa connaissance et de lui offrir mes bons services.

Vos magnifiques "*Planta Hightland*" me sont bien venues, avec le bon cadeau de *Flora of N. B.*, qui avait trop longtemps manqué à ma bibliothèque. J'admire votre activité jamais fatiguée! Merveilles aux services importants que Vous rendez à notre Science! — mille remerciements pour le bon bouti que Vous me tenez! —

Dernièrement j'ai eu la visite de M. Dorel avec sa femme et enfants de M. Martin avec sa famille. La *Flora de Brésil* leur a beaucoup intéressé. Contenu par ces deux, M. Dorel remplaçant le plan de M. Lindley. Les affaires politiques ont interrompu la publication. Je continue à collaborer. Les *Papaveres* et *Chlor.* ont déjà paru et les *Histories* de la *botanique* sont sous presse. *Mertens* publie la *Polygonia*, *Flore* les *Nyctaginées*. La *général* la publication sera effectuée prochainement.

J'ai joint ici les *Letters of Leding*. Le cartou est adressé à New-York, chez M. G. V. Putnam. Avec le bouti de bon cœur.

Avec, mon cher Docteur, mes sentiments d'estime et d'amitié sincères.

Tout à Vous

L. B. Halliguer

WM

11/18 Dr. Wa

Fisher Professor of Natural History
in Harvard University

Cambridge

Massachusetts, U.S.A.



à français.

J'ai étudié avec la plus
grande satisfaction votre admi-
rable mémoire sur la végétation
du Japon. Le genre d'étude des
plantes a toujours vivement intéressé,
c'est le chemin à des grandes
découvertes.

Vous remarquerez que j'ai joint
ma proposition à Amsterdam,
pour prendre la chaire de Bota-
nique à l'université d'Utrecht.
Je me trouve très-heureux dans
ma nouvelle position.

Et sans Valer et
avec ses amis
Adieu

J.H.W. Miquel

Mr de Goy
Cambridge

Utrecht, Mai 1861

Cher Ami et très honoré
Collègue!

C'était, si je ne me trompe
pas, ma faute, que notre corres-
pondance s'est endormie depuis
longtemps déjà. Vos bienveillants
envois de différents brochures et
livres s'en sont trouvés cependant,
que votre amitié n'était pas
perdue pour moi, et quant
à moi, je puis bien assurer,
que mon silence était par-
faitement involontaire, c'est

à dire une des tristes limites
d'occupations trop nombreuses. —
Permettez-moi donc me permettre, de
corriger ma faute et désigner
ces envois présents de
quelques plantes de l'Archipel
malais. Vous y trouverez
plusieurs échantillons fort
petits, très chétifs, mais comme
~~ils~~ viennent de si intérieurs
de Sumatra, où le voyageur
est exposé à des grandes
difficultés, de la part de la
population et du climat,
il est difficile d'avoir
des échantillons brillants.

Les genres et espèces nouvelles
sont décrites dans le 1^{er} Supplément
à ma Flora des Indes Orientales;
traitant sur la végétation de
l'île de Sumatra, ouvrage qui
paraîtra aussi en allemand,
comme un livre à part.

Je continue encore toujours
mes études sur la riche Flore
de nos Colonies Indo-orientales,
de sorte que plusieurs types
meuriront encore. —
Dès le courant de ce mois
je publierai aussi le premier
numéro du Journal d'Orbigny —
sur l'Inde Orientale, rédigé

Cher ami ! Je viens
d'expédier une caisse
de plantes pour votre
Herbier & Vous trouverez
ma lettre dans la
caisse. Mais à présent
je suis sûr qu'il est encore
nécessaire de Vous faire
part du Bill of Lading

qui te vi-vois, b'g'ni

l'in le nom de M. d'Am

B'nt, ton jardinier.

h. a. V

J. A. W. Alligues

W. d'Am

3 leai W. d'Am

Utrecht 17 Dec 1866

voyez donc, cher ami, que bien
des diffusions se rattachent à la
direction de l'herbier de Leyde. —

Je trouve parmi les pl. du Jardin
beaucoup des types de l'Himalaya,
aussi de l'America, mais plus
souvent des espèces voisines, sans
être strictement identiques,
"petits vicariats", probablement
— Vous êtes Dorrisme — les
provenant des mêmes caudexes.

Adieu! Croyez moi toujours
votre dévoué ami.

F. H. Mullig

Mon cher ami!

Depuis quelque temps je suis
dans la possession de votre
magnifique volume, le Retour
de l'Exploring Expedition. C'est
un ouvrage d'un haut intérêt
pour notre science, un de ces
ouvrages classiques dont nous
continuons à enrichir la Bota-
nique. — Recevez mes remer-
ciements bien sincères pour
cette nouvelle marque de votre
amitié.

Mes envois de plantes et de
livres s'envoient probablement

des vos mains. Le prochain envoi
de doubles sera exclusivement
composé de plantes du Japon.

Je m'occupe également à déterminer
les masses énormes de plantes ^{primaires}
qui ont accumulé depuis 40
ans dans notre Musée.

M. Mettenius, Mettner et quelques
autres amis sont des précieux

collaborateurs pour moi; mais
la plupart des personnes qui se
nomment mes collaborateurs,

sont cela seulement par nom,
et pas du tout en réalité.

Aussi je dois faire presque
tout personnellement. P. ex.

M. Bück, ancien d'herbier,

voulait déterminer nos Graminées,
et dans l'espace de 2 1/2 ans
il n'a pu encore déterminer
une seule espèce.

Le Gouvernement a décidé
que l'Herbier de Jussieu
sera réuni au Musée botanique.
En prenant une revue de
cette collection, je trouve
que la précieuse suite des
Lichens, publiés dans les
P. Jussieu, y manque! M.
de Borch les avait dans
la maison; après la mort
ils ont tombés par hasard
dans le panier de M. Bück,
qui achète partout des herbiers,
et maintenant on dit, que ce
n'est qu'un gros des doubles! - Non

Amund
Febr. 2.

Utrecht Janv. 1864

1864 aura la fin de cette
lutte si triste, ^{mais sous}
condition, que le beau et
vrai principe de la liberté
y sortira avec gloire!
Oùl glorieux événement
ce serait, si l'abolition
de l'esclavage serait le
dernier acte de ce drame!

Adieu, cher ami!
Me crojer toujours votre
tant dévoué ami.

F. H. Welligweg

Cher ami!

Dans ma qualité de Directeur
de l'Herbar de Leyde je viens
d'expédier une caisse à votre
adresse, contenant des échantillons
doubles de cette collection. Je
me propose de la faire suivre
par d'autres envois, car la
quantité de pl. doubles et des
plantes par envoi examinées y
est énorme!

Or, quoique cela n'est pas une
condition, je serai pourtant
fort content, si vous pourriez
nous communiquer quelques

chose,

choix des richesses botaniques
dont vous disposez.

Je suis maintenant Directeur
de deux Musées botaniques,
1^o du grand Musée botanique
de Leyde (où je me rends
chaque semaine pour un ou deux
jours), et 2^o de l'Hortus acad-
emique de l'Université d'Utrecht,
où est mon domicile. —

Je m'occupe à distribuer
les herbes énormes des doubles
de Leyde aux Musées de
l'Europe et à des Botanistes
distingués. — Plusieurs amis
m'écritent dans ces années
des fl. pas encore déterminées,

et qui depuis 30 ans y ont
été conservés. Les herbes
de nos recherches se publient
dans les Annales Mus. bot.
L.B., dont j'ai examinées
plusieurs exemplaires, par
l'Ambassadeur des États Unis
à La Haye, destinées à
l'Académie des Sciences et à
la Société de Botanique de
Paris.

Les tristes circonstances
de votre patrie ont sans
doute la cause, que j'ai
été privé depuis longtemps
de vos nouvelles. J'espère
de tout mon cœur, que ^{l'été} l'été

Utrecht 5 Mars 1864

To the American Academy of
Science & Art, under patronage of
Mlle. de Ligne des de Mémoires
et proceedings, et des très honorables
pour ce cadre. Mais dans le
moment le Mlle. de Ligne peut donner
de la part que les Académiciens. Une
exception a été expédiée à l'Académie,
si ce n'est pas par la Légation
Américaine à la Haye.

Pour les envois de votre part je prie
d'abord de faire séparément ceux
destinés pour la Mlle, et ceux desti-
nés pour moi. La manière d'expé-
dition est signalée sur un billet joint
à cette lettre.

Orant en vol. 7-10 de H. de Ligne, je m'informe
en cher un antiquaire. —

Yours truly, mon cher ami, "a very happy
letter". Mais dans le moment je suis
extrêmement occupé. Prochainement vous
avez une lettre plus détaillée et plus
nettement écrite! — Votre Photographie
a comblé la joie que vos lettres m'ont
causée. — Adieu, me croyez toujours
votre ami. Sincèrement

J. H. Mullens

Mon cher ami!

Vos deux lettres m'ont comblé
un très grand plaisir, d'abord
parce qu'elles me rassurent parfai-
tement à l'égard de votre
santé et la continuation des
vos travaux scientifiques.

Sur la politique américaine nous
sommes parfaitement d'accord,
et j'espère que dans le courant
de cette année je pourrai vous
féliciter avec la victoire de
Napoléon sur le Sud, de la liberté
sur l'esclavage.

Je me fâche beaucoup, que mes
caisses de plantes aient tant de
dellars! C'est le malheur des

Hommes trinitaires d'être exploités
par les spéculateurs. — Mais
je fais mon mieux, que cela s'arrange
plus, et que la seconde édition
soit transmise à moins de frais; —
par la voie que Vous m'indiquerez
ou par une autre encore moins
chère.

Tout ce que Vous pourriez communi-
quer au Musée botanique de Leyde,
sera reçu avec la plus grande
gratitude. Les plantes de S. Pauli
Esq. et celles du Japon seront
des vrais trésors pour nous.

Au nom de notre Gouvernement
j'ai envoyé plusieurs exemplaires
des Annales Mus. L.R. à différents
Institutions et Sociétés savantes
chers Vrs.

A notre second envoi j'ajouterais
de ma part un exemplaire des

Annales Mus. L.R., comme
souvenir d'amitié pour Vous,
mon cher ami! Si de votre
part Vous pourriez me fa-
voriser d'un exemplaire de
Botany of S. Pauli Esq.,
Vous compléteriez une grande
lacune dans ma Bibliothèque.
Veillez alors adresser ce paquet
directement à mon adresse à
Utrecht, ou est mon domicile,
pendant que le Musée bot.
qui je désire est à Leyde,
où je me rends chaque se-
maine. J'ai aussi la direction
de l'Herbier de l'Université, au-
tant pour mon herbier. Je
prends en même temps le plaisir
d'ajouter ici une liste des grands
ouvrages de votre main, que le
don d'amitié, pour
Vous faire voir à quel terme de la
populaire.

P.S. du 21 Mars. J'ai pris des informations
sur la meilleure manière de nos expéditions,
chez un Montier de ma famille, Marchand
à Rotterdam. Celui-ci s'est mis en rapport
avec le Consul des Etats Unis, qui a dit que
Vous avez payé une si grande somme d'argent
probablement à cause des impôts. Mais comme
des objets destinés à la Science peuvent être
libres de ces impôts, on pourra des milliers
de livres à cet égard. - Mon ami à R., Messieurs
Warkiter & fils expédieront mes caisses

à leurs Correspondants à New York, et en
les. Mais les feuillets parvenus en recu-
vrant les lettres pour les envoyer à
Rotterdam. - Aussi - dit-on - entre
Correspondance des. Sur et bon marché!

Aussi tt

Le posside

General R. of the U.S.
Vol. I & II.

Towney & H. Geo, Thos Geo. Co.
Vol. I. & Vol II part. I-3.

en outre je posside d'avoir
tous les memoirs ~~publies~~
dans le American Academy,
et les brochures tirees des
Proceedings.

Mais je ne posside pas les
Vols of Pacific R. Road Journal;
c'est bien un bonheur pour moi,
de les posseder.

Utrecht le Sept. 1864.

Mon cher ami!

J'ai le plaisir de Vous
annoncer qu'un envoi de nos don-
bes vient d'être expédié à
Votre adresse par M. Wachtel
ou fils de Rotterdam via New-
York. Je pense que par cette
voie les caisses Vous parviendront
bon marché.

Je n'ai pas encore reçu le
paquet qui contient des chales
pour moi et pour M. Breda. On
m'a bien annoncé de Leipzig,
que je pourrais le recevoir en

payant une certaine somme
d'argent. J'ai offert de le
payer, mais n'ai obtenu cela
le paquet n'est pas encore
arrivé.

Pour l'avenir je Vous prie,
ce qui concerne nos envois
mutuels, de les expédier
via New York par les corres-
pondants de mes amis, Washburn
et de ne jamais ajouter aux
paquets destinés à moi des
choses pour d'autres corres-
pondants, d'adresser les choses
destinées pour moi directement
à moi, et les envois destinés
pour le Mlle de L.

directement à l'adresse de
la Mlle. En faisant
autrement, il y aura tou-
jours de la confusion. -

Le dernier envoi des Annales,
vous l'en avez déjà
parvenu. Les fascicules IX & X
viendront prochainement.

J'espère avec la troisième
distribution de nos doubles
avoir envoyé une bonne
quantité de pl. japonaises. -
Je m'occupe maintenant avec
leur détermination. Nous avons
une grande quantité de doubles.

Adieu, cher ami!

Me croyez toujours votre
sincère ami

J. H. Willig

Utrecht 30 Mars 1866

lettre, Vous me ferez un grand
plaisir.

Je vous salue cordialement

otre dévoué ami

F. A. Willigen

Cher ami!

J'ai signé votre lettre d'introduction
pour le Dr. J. pendant une semaine
très bonne, maisheureusement
valable. j'ai encore eu le grand
plaisir de le recevoir et de faire
sa connaissance, qui m'a beaucoup
intéressé et entre autres nous
avons beaucoup parlé sur Vous.

Maintenant je prend la liberté
de m'adresser à Vous avec quelques
questions. Le Dr. J. de son Poulis
Flora Jy. Vous montrant que j'ai
continué mes recherches sur cette
Flora et je suis heureux de fournir
mes communications que toutes nos

collections

collections japonaises sont déterminées
et décrites, excepté les Lichens
et quelques Champignons. Pour
ces derniers nous avons L. C. G. H.
à Mr. Berkeley, mais pour les
Lichens nous avons rien eu.
Vous dites dans votre excellent
mémoire qu'il y a manque de
les examiner. Mais où les
spécimens de cet examen sont
publiés? Au moment où M. B.
Black a rédigé le catalogue
de la Flore du Japon, rien n'en
était publié, mais peut-être
cela a été fait après. —
J'ai trouvé au genre *Vicia* de

votre *Heliosiphis* et je l'aurais
eu identique et presque conspé-
cifique, mais les graines de votre
genre semblent d'après votre
description totalement différentes;
les semences de mon genre sont
des "Semins adhérents... imbricés
manuscrits linéaires, ~~sup.~~ sub-
plans, très tenus lada utraque
extremitate longe alata"... —
Lythrum grandiflorum Pers. le
trouve aussi dans l'île de Nippon. —
Aussi j'ai retrouvé le *Melanthium*
luteum Th., qui paraît être une
espèce fort rare. L. M. promise
~~notre~~ m'envoy. ~~de cette lettre~~
un petit échantillon de *Chamelirion*
luteum (qui me manque) dans votre

Surtout de Nord du Japon, les autres
de la partie moyenne et du Sud.

Quant à votre Pythochara (1)
dentata j'ai peut-être si elle pourra
être l'origine de Ligularia tibetica.
J'ai rencontré une grande quantité
au N. de la Sibirie. — Une
Crocus peu différent de la forme
flor!

Adieu, cher Gray, me croyez
toujours votre sincère
et reconnaissant ami

F. H. Miquel

(1) J'ai décrit ce genre au
Ligularia

Albert P. Hall, W.C.

Cher ami!

Longtemps déjà j'avais du vous
l'écrire et vous offrir ma profonde
gratitude pour la magnifique envoi
des excellents volumes de "Reports",
un des nombreux monuments de l'éner-
gie américaine! — Mais tout est
cher ami, j'ai peut-être souvent à vous
à l'empire de ces grands événements
politiques qui se sont passés chez
vous. — Vous connaître à cet égard
mes sentiments et vous pourriez
vous figurer ma joie sur la
victoire de la bonne cause, de la

liberté sur le tyranisme, de l'humanité
sur l'enslèvement. Cher nous, peuple
libre, existe une sympathie extraordi-
naire pour vos affaires, et nous
avons partagé cette profonde douleur
sur le sort de grand et noble
Lincoln.

Pour l'humanité entière le sort
de votre cause est de la plus
grande importance; car les États-Unis
sont en vérité les défenseurs de la liberté
dans le monde. La joie du tyranisme
sur les malheurs que nous avons éprouvés
au commencement de la guerre, le
démontre et je pense que sous
d'autres circonstances on n'aurait

pu voir d'établir une nouvelle
Pologne dans le malheureux Schles-
wig et de maltraiter le libre
peuple du Danemark. —

Mais revenons à la Botanique.
Je n'ai d'espérer à votre égard
des fascicules du Rhynchospora, et en
plus de temps j'en ai une copie
pour le Japonais et indienne.
J'ai presque fini l'examen de
vos lettres du Japon et j'en
commence la publication. Pour
vous faire une idée de notre
collection, permettez que j'ai trouvé
p. ex. 120 complices, 50 Labellies. —
Je n'ai pas retrouvé toutes les
espèces que vous avez indiquées, sans
doute parce que vos collections provenaient

Wheat Dec. 1866

Cher ami !

Je profite de t'envoier des 8
dernières livraisons du second
volume des *Annales*, pour t'en
offrir mes remerciements bien
sincères pour la bienveillante
communication des échantillons
de *Melanthium* et de *Helio-*
repus, qui m'ont fait beaucoup
de plaisir. Le dernier genre,
quoiqu'entièrement du même
port, offre par les semences
beaucoup de *Sugesthia*. - Cher

Am^s

Professor Asa Gray

Cambridge

au Melanthin Vous trouverez dans
la prochaine livraison le résultat
de ma comparaison.

M. Paulin F. Jyr., éditeur
de Paris, Vous prie de à une
autre copie. J'ai à
terminer d'abord et ensuite
pour Vous l'envoi du complet.

Agréez cher Lili
l'expression de mes sentiments
d'estime et de 'amitié'
d'union.

Avec à Vous
F. W. Mignot

und
Oct. 17

Utrecht juillet 1867

Mon cher Collègue!

Monseigneur le S^{te} Général

Professeur de la Gray

Prof. de Botany

Cambridge

Massachusetts

United States

J'ai le plaisir de vous envoyer
ci-joint les notes des Annales
et une petite brochure. Le peu
de temps vous accorde au sujet
mémorise plus détaillé sur la
Géographie botanique de Japon,
aussi en français.

Ma Prolepse sera terminée dans
le courant de l'été et je
vous enverrai alors les numéros.

Mais j'ai oublié quel fut
X le dernier numéro de cet
opuscule, que vous avez vu et
si

Si cela vous paraît fâcheux, je
vous prie de vouloir m'indiquer
le dernier numéro qui vous
est parvenu.

Enfin je vous envoie beaucoup
pour la commission de Cha-
melin; l'épave du Japon
ne diffère pas beaucoup,
mais elle est hémisphérique,
celle de l'Amérique étant
physiologiquement droite.

Avec mes salutations cordiales
et sincèrement amicales

Très à vous

F. H. Meyers

Année
Nov. 25

Utrecht 7 Nov 1867

Cher ami,

Je viens d'expédier les premiers
de *Prolesia Fl. Jap.* qui Vous manquent
encore et qui constituent l'ouvrage.
Si j'ai osé de dire à Vous mon
petit questionnaire (Sans Vous en demander
la permission), Veuillez - je Vous
prie - l'attribuer à mes senti-
ments très sincères d'estime et
d'amitié.

Un petit mémoire qui traite de
géographie botanique du Japon en
général livré en quelques jours.

Je me réjouis beaucoup de
attendre Vos communications ultérieures.

que Vous m'avez écrit dans votre
lettre. Tout ce que vous m'avez écrit de votre
lettre m'intéresse toujours vivement.

Il y a-t-il chez vous un ouvrage
ou des mémoires sur les plantes
tertiaires trouvées dans les dépôts
de votre pays? J'ai l'idée que
parmi la Flore de l'Amérique du
Nord et celle de l'Asie orientale
ont eu des liens plus intimes
encore.

Mon libraire me prie de m'in-
former si un de vos libraires
solides voudrait se charger
en relation avec lui et se
charger de la vente des livres
qui se publieraient chez nous? Si

Vous pourriez m'indiquer quelqu'un,
cela me serait très agréable.

Avec mes salutations cor-
diales je suis

Votre très ami

F. H. Wallingford

Mitten
Hurstpierpoint
30th Oct 1889

Dear E. Gray,

I sent addressed to you
on Thursday afternoon last
a parcel containing everything
Mr. Sullivan desires to see
and by this time I expect the
Railways will have delivered
it to you safely.

The parcel contains all I have
of the Mosses he wishes to
examine and I only regret
that I have nothing more
at hand ready to send him
beyond a small collection of
Sarracenian specimens which is

included in the parcel.

I regret to find that I overlooked in your note of 4th Oct. that Sullivant wished to have a set of Spruce's Mosses, I ought to have written to you at once to tell you that I did not have anything to do with the disposal of these, Spruce did this himself. I assorted and catalogued all the specimens and made up a certain number of sets, the duplicates were all here until quite recently, but they have now been sent to him. I will write to him about this matter.

I send with this a letter from Mr. Sullivant which will tell him all about the contents of the parcel and you can forward it to him at your convenience.

I am sorry that no opportunity has offered me the pleasure of having seen you and am

Dear Sir

Yours Truly Yours

William Miller

those who know the history of the
older collections described and distrib-
uted before my time, and very long
before theirs, and the packet should
come back direct to me.

I have lately been waiting, before I
replied to your letter, that I might be
enabled to send you a copy of my
paper on Japanese mosses, but although
Mrs Britton told me she had seen it being
packed up at the Linnean Society's
rooms, I am still without it, it has
been four years in their hands. When
putting this paper together I found
almost insuperable difficulty in trying
to identify the *Hypna* described from
that region by Sullivan and Lequeux.
I think from Wilkes's Voyage, the descriptions
are short and drawn up when everything
was *Hypnum*, is there any residue of these
mosses from which I could have a sight
of sufficient to identify them and restore
them to their proper places? fragments would
do, probably. I have most of them, but am
unable to know them. Since writing the
above I have looked at the title of the paper
and see it is the *Ephebe* under Capt. Rogers,

Hurstpierpoint
Sussex England
10. Sept 1891

Dear Mr Watson,

I owe you many thanks for
the copy you so kindly sent me of
your book on American Mosses,
what a fortunate chance it was
that Lequeux obtained your assistance,
nothing so original and independent
has before been done, of course in
all books of this kind it is possible
to say here and there something
might have been put different, but
that does not at all detract from
value of your remarks in our own
good English, which is often more
taste than the regulation Latin ex-
pressions. I am here and
spent a Sunday here on his way
to see Schimper I think, at that time
he seemed in pretty good health
I had but little correspondence with
Sullivan being, in fact, always over-
done with other matters, and so I have

W. Britton

continued to be until the last two or three years, so many specimens came to me from Kew, that I have been kept fully occupied; for I have only the intervals between business matters that sometimes consume all my time and then I get too fatigued to do much good. About three years ago having to move my collections from one room to another I determined to overlook all the surplus of things that had accumulated for many years: for I was sure many things had been overlooked for want of knowledge and this I am now about two thirds advanced in, for I get on but slowly.

It was Gray when he was here at Kew who applied to me for the mosses for Sallivant and I sent them to him. Several were figured in his Icones and when he died Gray found the packet and in a note promised to return them on the first suitable occasion, my own impression, until your letter

came, was that he had sent the packet to Kew and that it was there put away and forgotten. I had experienced something of this kind before. The specimens were mine and had never been in the Kew Herb^{um} the most interesting having been overlooked as common species by Hooker and Wilson before my time. I have two sets of Drummond's N. American Mosses which being unlike I have to keep both, Douglas's names some of which Sir W. Hooker figured in his Musc. Exot. were taken from the collections belonging to the Royal Horticultural Society, many years after all their dried plants were sold, and this collection was sent to me to put names to the specimens and divide them for a dealer who had bought them to make what he could by them; thus I got some specimens which probably no one else had seen, and many that were unknown except by Hooker's figures. I mention all this because Mrs. Britton talked about the specimens lent to Gray when she was at Kew and they have since asked me about them; but there is no one

and not Wilkes, it is in the Proceeds
of the American Acad^y of Arts & Sc. Vol
IV. 1857-1860 p. 275 I have the paper
but copy from a des list, I was obliged
to give up the three species of Fissidens
as hopeless. I think all the Hypnum
are in doubt the whole number of species
mentioned is 33.

I have still by me one or two copies
of my Austro American mosses and
some other small papers also some
of Powell's mosses from Sarroa, he
being dead I shall dispose them
they are not all fine specimens
Powell himself not studying them
but only admiring them and gathering
them because I asked him I have also
a good many Australian things if
I have any thing worth your acceptance
I shall be very glad to make some
return for the work you so kindly
sent me.

We have experienced a cool and
dull summer this year, a whole fine
day about once in three weeks, just
now it seems improving and we

have had four days with sunshine and
at last it is warmer, several American
plants which I hoped to flower have been
lost from the continued wet. Phacelia
seem to have twice saved until the
second year and then look they go
wrong in the central bud; there is however
always something to find out by those
who try to grow plants

With best respects

Yours ever

William Witten

I shall probably take
a French mail steamer from
Marseille on the July 28th &
shall then be in Tokio on the
6th of Sept.

Please remember me to
Mrs. Gray.

Very faithfully yours
King's College

Queen's
Asplenium

11 Montague Street,
London W. C.
June 23rd. 1889.

Dr. Seward Watson,

Dear Sir;

I arrived safely
at London last Monday afternoon,
I have been to Kew almost
every day and spent entire day
there. I have met Mr. Dyer,
Sir Joseph Hooker, Prof. Oliver, Mr.
Baker, Hemslay, Clarke, Brown,
Hooker and Masson. Prof. Oliver
has been very kind in showing me
any thing I requested him to
examine.

I went yesterday afternoon
to South Kensington Museum
to see Mr. Carruthers and others.
Being Saturday, and also a little
past four, they were all gone.
I am intending to leave
England tomorrow evening for
Berlin and St. Petersburg.

I left my paper in such a ^{bad} state entirely under your care, that I do not know how and when I can repay for all your kindness. I undertake this unpleasant task. I found here that Hemsley's Chinese plants have been already published as far as Euterpnaceae.

If my paper is not ^{yet} entirely set in type, I wish to make a few corrections. I found that *Quercus mongolica* is little different from *Q. grosseserrata* in the characters of scales of cups and also in leaves. I have been able to examine a large number of specimens from Manchuria and China, showing a great similarity between them, but evidently distinct. My plant is then *Q. grosseserrata* with not *mongolica*. Please retain ^{and note} synonyms relating to the former. ^{you cannot} ~~there~~ in connection with Range *mongolica*. Range of distribution is to be altered. *Sachalin, Manchuria*

via, Selkirk, Mongolia, Manchuria means the *tabernaemontana* and are sure that they will prove one day the varieties of *tabernaemontana* species. If you put in a few words as to their similarity, I shall be very much obliged.

The next plant is *Cephalanthus chinensis* (Baker). Found that *Cephalanthus chinensis* (Baker) is identical with *Cephalanthus chinensis*.

Please put these names together. I remember rightly, *Selkirk* in his flora. *Sachalinensis* but it is right form. (second). I know that *Cephalanthus chinensis* is identical with *Cephalanthus chinensis* as a synonym. Range *Sachalin, Manchuria, Mongolia, China*. *Sachalin* extends to *Sachalin* and *Manchuria*. As to the rest I do not see any important points that should attend.